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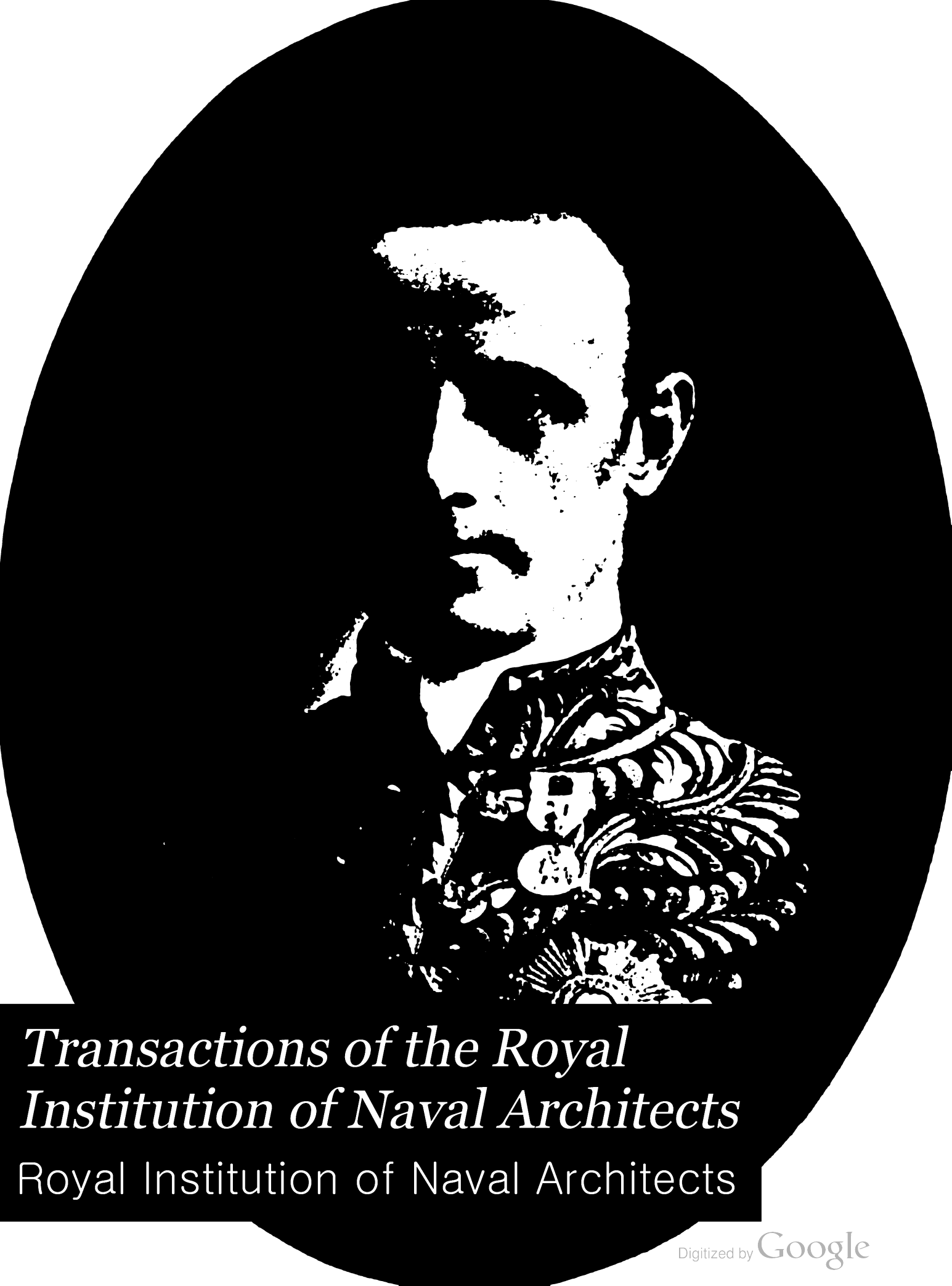

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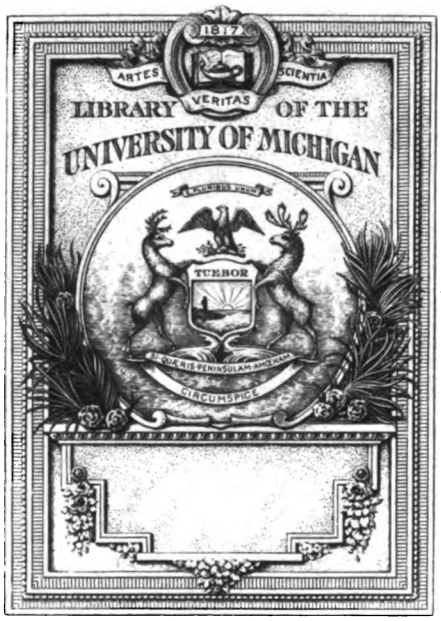

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Horn, Andrew
Hovgaard, G. W., Commander, R.D.N.
Howaldt, A. I. C.
Howaldt, G. F.
Howden, James
Huddy, J. B.
Hudson, C. E.
Hudson, Henry
Hughes, John G.
Hughes, Thomas W. R.
Hughes, W. O.
Hume, Graydon
Hume, James H.
Humphrys, R. H. (*Vice-President*)
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Hunter, James
Hunter, Joseph G.
Hunter, Summers
Hutcheson, Archibald
Hutchison, Graham
Hutchison, John B.
Hutchison, J. H.
Hyde, John S.
Hyne, A. E., Eng. Lieut., R.N.

Idle, George
Inglis, James Denny, B.Sc.
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Ingram, H. P.
Irving, John
Irwin, J. H.
Isaac, Percy L.

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Isakson, Karl J. A.
Isherwood, J. W.

Jack, James R.
Jack, J. W.
Jack, W. C.
Jackson, William
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James, M. C.
Jansson, Konstantin
Jenkin, T. H.
Jeroniti, L. L.
Johns, Arthur W.
Johns, H. E.
Johnson, Alex. B.
Johnson, E. S.
Johnson, Johan
Johnson, T. Allan
Johnson, William
Johnston, David
Johnstone, George, Lieut., R.N.R., F.R.S.E.
Jones, A. L.
Jones, Arthur P.
Jones, T. Turton
Jordan, Charles H.

Kay, Alexander J.
Kelson, George
Kelson, George Henry
Kennedy, Sir Alexander B. W., Professor, F.R.S., LL.D. (*Mem. Council*)
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Kerfoot, James
Kidston, John B.
Kincaid, J. G.
King, Frank B.
King, J. Foster (*Mem. Council*)
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Kitching, Alfred
Kitching, J. F.
Klock, Christoph F. H.
Knight, C. Wm.
Knight, George S., jun.
Knight, Thomas H.
Koch, J.
Kondo, Motoki, Admiral, Constructor, Imperial Japanese Navy
Konishi, Shinzaburo
Kops, J. T. J. de Bruyn
Kraft, F.
Kurobe, Hiroo, Rear Admiral
Kwong, Wong

Lagane, A. J. A.
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Langton, Alfred
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Lawrie, W.
Lax, G. A.
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Lecointe, Albert
Leetham, James H.
Leflaive, L. J. G.
Lendecke, Gustav
Leslie, Robert
Lester, Henry E.
L'Estrange, A. H.
Lewis, William
Lilliehöök, H. H.
Linnard, J. H.
Linnington, Edward A.
List, John
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MacDonald, D. R.
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McIntyre, Matthew
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McKellar, Duncan
Mackenzie, James
Mackenzie, John G.
Mackie, W. A.
Mackintosh, A. R.
Mackrow, Clement
Mackrow, G. F.
McLachlan, John
McLaren, W.
McLaurin, John
Maclean, Andrew
Macmillan, William
McMillan, Wm. M.
McNeil, James
MacNicoll, Nicol
MacPherson, John
Macwilliam, John
Maddison, Joshua
Maginness, E. John, M.V.O.
Magnussen, Neils M.
Maier, Fritz F.
Main, Archibald M.
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Mancor, James H.
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Mason, George F.
Massa, Carlo
Matthews, Robert
Maxton, James
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May, W. Walker
Mayston, Robert, Engineer Rear-Admiral
Meerten, H. Van
Meldahl, K. G.
Meldrum, M. S.
Metcalf, H. E.
Meuwissen, J.
Meyer, J. L.
Middleton, R.A.
Millar, Jackson
Millar, Thomas
Millard, J. W.
Miller, G. McEwan
Miller, Philip L.
Mills, John
Milne, W. J.
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Milton, J. D.
Milton, J. T. (*Vice-President*)
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Mitchell, Ewen H.
Mitchell, John A.
Mitchell, R.
Mizutani, Y.
Moffitt, George
Moffitt, Robert
Möller, W.
Mollison, James
Monroe, Robert
Monroe, W.
Mooney, Edgar N.
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Morrish, S. W. Furze
Morrison, Roderick
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Morton, Robert
Muir, Hugh
Muir, J. E.
Muir, R. Home
Mullan, William
Mumford, A. G.
Mumford, E. R.
Mumme, Carl
Munday, C. F.
Munn, Robert A.
Murphy, B. S.
Murray, Alexander
Murray, C. W.
Murray, Henry
Myles, David

Nakashima, Y., Eng. Capt. I.J.N.
Napier, Henry M.
Narbeth, J. H., M.V.O.
Nash, S. A. G.
Newman, Richard Lano
Nichols, C. B.
Nicholson, C. Ernest
Nicholson, P. F.
Nicol, George
Nicol, John
Nielsen, K., Captain
Nightscales, J. H.
Nixon, William
Norman, W. H.
Norris, A. E. F.
Norris, William James
Norton, Harold P., Commander, U.S.N.

Oglethorpe, W. A.
Oldham, Samuel C.
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Orlando, Salvatore
Orr, Alexander T.
Oswald, A. L.
Oswald, T. Ridley
Ottewill, James T.
Owens, Thomas G.
Oxford, Benjamin G.

Pagel, Carl C., Professor
Parlett, Harry E.
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Paterson, John
Paterson, John
Paterson, Richard
Patterson, J. Lewis
Pattison, Alfred R., Engineer Commander
Pattison, John A.
Pattison, Joseph
Peacock, Edward L.
Peacock, James
Pearce, E. A. J.
Pearson, Frank H.
Peck, James J.
Peck, N. E.
Peck, Robert R.
Penney, R. H.
Perrett, Josiah R. (*Mem. Council*)
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Pethick, Philip L.
Petree, James
Petroff, A., Lieut.-Colonel, I.R.N.
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Piaggio, Carlo
Piaüd, Pierre Léon
Pickersgill, Frederick
Pickersgill, William
Pierrard, A.
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Piper, James R.
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Pohl, R.
Pollock, David
Pollock, James
Pollock, John
Pollock, Walter
Pollok, R.
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Portella, J. M.
Potter, G. Herbert
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Preston, John
Proctor, William E.
Pruneri, Giorgio, Colonel R.I.N.
Pryor, Benjamin
Puga, Cayo
Purvis, Frank P.

Quelch, R. J.

Radford, R. H.
Ramage, A. G.
Ramsay, Charles T.
Rasmussen, Captain A., Royal Danish Navy
Ratnyk, Xaverius, General
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Raymond, E.
Rea, H. E.
Read, J. T., jun.
Readhead, James
Readhead, W. B.
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Rechea, Miguel
Redman, Joseph F.
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Reed, T. Alfred
Reeder, W. C.
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Reid J. Gardiner
Reid, John
Reilly, M. O.
Relf, J. T.
Rennie, George B.
Rennie, J. Assheton
Rennoldson, Charles
Rennoldson, Joseph Middleton
Reynolds, C. H.
Richards, A. E. G.
Richards, George B.
Richards, John J.
Richardson, Edwin B.
Richardson, John L.
Rider, S., Eng. Commander, R.N.
Ridsdale, H. Wheatley
Rinesi, Giovanni
Ritson, Maurice
Roberts, I. E. S., Eng. Lieut., R.N.
Roberts, William C., R.N.R.
Robertson, Alexander
Robertson, Duncan
Robertson, Graham
Robertson, Leslie S.
Robinson, J.
Robinson, Mark
Robson, George
Roeis, A.
Roeis, Peter
Roffey, Charles E.
Roger, George W.
Rogers, Herbert M.
Rogers, John
Rolland, Alexander
Roome, G. W., Eng. Commander, R.N.
Roper, Leopold
Ropner, Leonard
Ropner, R., jun.
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Roscoe, Peter H.
Ross, A. J. C.
Ross, William
Ross, William
Rota, Giuseppe, Lieut.-Colonel, R.I.N.
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Rowell, Herbert
Royal, J. G.
Rubie, Charles Andrew F.
Ruck-Keene, H. A.
Rudd, Charles, Engineer Rear-Admiral
Rugeroni, A. E.
Rundgren, K. E.
Rusden, Lowry
Rush, H. C., Eng. Comm., R.N.
Russell, James
Russell, Joseph
Russo, G., Major, R.I.N.
Rutherford, Alex.

Sachsenberg, Gotthard
Sadler, Herbert C., D.Sc.
Salter, T. C.
Salvesen, Henry A.
Sampson, A. W.
Sampson, John
Samson, P.
Samuda-Benthall, J. L.
Saunders, Samuel E.
Sawyer, F. H.
Schiestl, L.
Schlick, Otto
Schnack, S. F. A.
Schneider, F. H. M. L.
Schneider, William M. F.
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Schofield, James
Schulthes, C. O. H.
Schulz, R.
Schunke, Hugo
Schütte, Johann
Scott, Charles C.
Scott, J. Edward
Scott, R. L.
Scott, Walter
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Seath, Thomas R.
Seaton, Albert E.
See, Horace
Seebeck, Georg
Selfe, Norman
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Sells, Charles de Grave
Shand, Hinton
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Shepherd, Francis J.
Shepherd, John W., F.R.S.E.
Shiba, Chuzaburo
Shield, Henry
Shilston, V. G.
Shiota, T.
Short, Joseph
Short, Thomas S.
Shotton, J. W.
Shute, C. W.
Sibbick, C.
Sigaudy, P.
Sillince, W. P., Eng. Lieut., R.N.
Sim, John A.
Simpson, Francis C.
Simpson, George
Sinclair, C. E.
Sinclair, Russel
Skentelbery, Charles
Skinner, James
Skinner, Leslie
Skinner, Thomas
Sloggett, George
Sloggett, T. H.
Smart, Alexander W.
Smart, Wm. H.
Smellie, John
Smith, C. E.
Smith, Frederick H.
Smith, G. Sampson
Smith, James A.
Smith, John
Smith, John
Smith, J. Reney
Smith, Stuart F.
Smith, S. Heckstall
Smith, Wasteneys
Smith, W. E., C.B. (*Vice-President*)
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Soliani, Nabor, Colonel, R.I.N.
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Soper, Joseph M.
Spence, William
Spyer, Arthur
Stafford, George H.
Stahl, Albert W.
Staples, S. F.
Steele, Robert
Steele, W. Ingraham
Steele, W. R.
Steinike, K. H. L.
Stephen, F. J.
Stephenson, Christopher
Stevens, Edwin A.
Stewart, Alexander
Stewart, Alexander W.
Stewart, James
Stewart, James
Stockwell, John A.
Stoddart, John E.
Stoney, B. Blood, LL.D., F.R.S.
Stratton. E. Platt
Stromeyer, C. E. (*Mem. Council*)
Strong, George H.
Stuelcken, J. C.
Sugitani, Y.
Sullivan, Clement E.
Summers, William
Suter, G. E., M.V.O.
Swainston, W. H.
Swan, Albert S.
Swan, C. Sheriton
Swan, W. Robert
Swann, William
Swensen, W. H. Carl
Swift, John Crane
Syme, James

Tagg, George J.
Tatham, Stanley
Tawresey, John G.
Taylor, Benson
Taylor, D. W., U.S.N.
Taylor, Matthew
Taylor, William I.
Tennyson, Constantin
Tennyson-d'Eyncourt, E. H,
Terano, Professor S.
Teucher, J. S.
Thayer, I. E.
Thearle, Samuel J. P. (*Mem. Council*)
Thom, John
Thompson, E. Routledge
Thompson, John Augustus
Thompson, W. B.
Thomson, Charles
Thomson, J. Auchterlonie
Thomson, James, M.A.
Thomson, R. H. B.
Thornycroft, Sir John, LL.D., F.R.S. (*Vice-President*)
Thornycroft, J. E.
Thumm, William F.
Tideman, Jan Willem
Tierney, Edward J.
Tilburn, William Alexander
Tilston, Henry E.
Timme, George A.
Tobin, T. C., M.A.
Toki, Y.
Torrie, William
Travis, Harry
Tree, Joseph
Trewent, Francis J.
Tritton, Seymour B.
Trowell, W. J.
Turnbull, George
Turnbull, Thomas
Turpin, James
Turpin, J. Christopher
Tuxen, I. C., Captain, Royal Danish Navy
Twaddell, James L.
Tweedy, John

Ubsdell, John A., jun.
Ullstrom, Otto

Valder, Arthur
Vallino, Mario
Valsecchi, Giuseppe, General, Royal Italian Navy
Van Veen, A.
Vaughan, W. S.
Veshkoortzoff, Peter
Vivet, Léon
Voss, E. C. A.

Wadia, Bomanjee S.
Walker, Archibald
Walker, Arthur H.

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Walker, John
Walker, Robert J.
Wallace, Duncan M.
Wallace, F.
Wallace, W. Carlile
Wallau, F. P.
Walpole, Thomas
Walter, C. O. M.
Walton, George L.
Warburg, C. G.
Ward, Herbert J.
Ward, John (*Mem. Council*)
Warder, G. E. E.
Warren, Frank Llewellyn
Warren, Frederick F.
Warren, Ruben H.
Warren, T. Samuel
Watkins, Alfred
Watkins, William
Watson, Ralph
Watt, Alexander J.
Watts, Sir Philip, K.C.B., F.R.S., LL.D., D.Sc. (*Vice-President*)
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Webb, Harry J.
Webb, W. H.
Wedgwood, A. D.
Wedgwood, J. C.
Weeks, Courtenay O.
Weighton, Robert L., M.A., Prof.
Weir, A. Frederick
Weir, George D.
Weir, James
Welch, John Joseph, Prof., M.Sc.
*Wellenkamp, H.
Wells, Thomas H.
Wemyss, G. B.
Wemyss, J. Leith
West, Ernest W.
Westgarth, Tom
Westmacott, A.
Wetherall, Thomas
Whayman, Wm. M., Engineer Commander, R.N.
Wheatley, W. C.
White, A. Francis
White, R. Saxton
White, Sir William Henry, K.C.B., LL.D., D.Sc., F.R.S. (*Honorary Vice-President*)
Whiting, William H. (*Mem. Council*)
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Widdas, T. Dodd
Widmann, Daniel E.
Wilkie, James B.
Wilkins, J. Edwin
Wikinson, Henri
Wilkinson, Thomas
Williams, Harris G.
Williams, Jesse
Williams, T. R.
Williams, Walter K., Engineer Commander, R.N.
Williamson, A. Smart
Williamson, Sir James, C.B. (*Mem. Council*)
Wilson, Robert
Wilson, W. Wills
Wilton, Bartel
Wilton, J. Henry
Wiltshire, George
Wimshurst, Henry C.
Wimshurst, J. E.
Wingfield, C. H.
Winstanley, P. D.
Winterburn, W. G.
Wise, Richard
Wisnom, Hugh
Wisnom, William M., Eng. Com.
Withy, Henry (*Mem. Council*)
Wolff, Gustav W., M.P.
Woods, Arthur R. T.
Workman, Frank
Wort, W. E.
Worthington, A. Martin
Wortley, H. B.
Wotherspoon, John H.
Wright, J. J.

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Young, J. Denholm
Young, Sidney
Young, W. Andrew
Young, William
Younger, A. Scott, B.Sc.

Zierold, John
Ziese, Carl H.
Ziese, Rudolf
Zimmermann, Rober
Zweig, Henry

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Allen, H. Pinny
Anderson, George

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Baker, Ernest F.
Barber, A. C.
Barnwell, F. S.
Baxter, Arthur W.
Beraza y Cortina, F. de
Böcler, H.
Bomphrey, A. M.
Brown, D. Alexander
Brundrit, Joseph
Bulkeley, G.
Bunday, William J.
Burgess, W. Starling

Casebourne, D.
Cawood, C. A.
Cochran, Alexander
Coller, Frederick E. W.
Cooper, R. P.
Cornbrooks, T. M.

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Crow, Lewis

Davidson, Charles I.
Denton, G. Price
Dickie, J. B.
Doxford, William
Duguid, R.
Dunn, J. R.
Dutton, Tom

Erismann, M. C.
Ermakoff, M. P.
Ewen, Cecil L.

Fittock, C., Jun.

Goodyear, Percy
Gow, George, Esq.
Gray, George
Griffith, Y. G. ap-
Guy, W. J.

Hall, Frederick M.
Harden, A.
Hardy, William
Hiley, A.
Hine, J. M.
Hitchins, Charles F.
Horn, William
Horton, J. C. V.
Houston, Charles E.
Howard, George
Howson, George

John, F. Armand
*Johnston, J. M.
Johnstone, John G., B.Sc.
Jones, R. Abbott

Kawada, T.
Kennaugh, T. T.
Kimura, Nario
Kinghorn, Robert E.
Kitsell, A. Edward
Knox, R., jun.
Kossura, P. S.

Lake, Richard
Lambert, L. Carlton
Lawrie, Peter S.
Leask, J.
Lindgren, Axel W.
Longmore, H. F.

Macfarlane, Archibald P.
McCallan, J. W.
McDonald, William, R.N.R.
McGovern, John
McIlwraith, Charles H. C.
McIvor, Benjamin R.
McKay, T. George
MacLeod-Carey, Arthur
McPherson, A.
Masters, Henry H.
Meldrum, Harrison
Middleton, Charles G.

Niblock, F.
Normand, A.

Paul, James
Payne, Arthur E.
Pearce, Francis T.
Phillips, G. W., Eng. Lieut., R.N.
Piercy, William
Polakov, N.
Powell, J. W.

Quelch, Arthur T.

Reiach, H. L.
Reid, Robert
Reith, R. B.
Richmond, J. R.
Ridell, Carl A.
Rigg, Ernest H.
Robertson, A.
Robertson, A. J. C.
Robertson, Hugh
Robinson, Ernest C.
Rosenius, H. D.
Runton, William

Sanders, T. Delve
Schippers, A. M.
Sexton, G. A.
Smith, A. E.
Solheim, J. J.
Speakman, Edward M.
Spence, F. Vernon
Stansfeld, L. D.
Steed, F. Herbert
Steele, James Edward, B.Sc.
Suzuki, Keiji

Tainsh, J. A. G.
Thompson, Joseph A.
Tickner, S. R.
Tsutsumi, S.
Turner, Alfred, Eng. Lieut., R.N.

Voos, Armand

Weir, William
Whitburn, Herbert L.
White, Hedley G.
White, P. M.
Wilding, Edward
Wilks, Edward C.
Wilson, A. H.
Woollard, Lloyd

Zimmer, A. H. A.

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Abdela, I. J.
Abel, Rudolph
Acland, Sir William A. D., Vice-Admiral, Bart., C.V.O.
Adams, E. Tarry
Agius, Edgar
Agius, E. T.
Ailsa, The Most Hon. the Marquis of
Ainsworth, G.
Alderson, Sir Henry, K.C.B., Major-General
Amsler, Alfred, Dr.
Angier, Sir Theodore V. S., Captain
Armstrong, The Right Hon. Lord (*Assoc. Mem. Council*)
Armstrong, F. H., Lieut.-Colonel
Arnold, W. F. A.
Ashton, Henry G. G.
Atkin, Walter S., Commander, R.N.R.
Aukland, Thomas F.

Bacon, R. H., Captain, C.V.O., D.S.O., R.N.
Baird, A. W.
Baker, R. Barrington, Lieut.-Col.
Bankson, Lloyd
Barge, L. G., Captain
Barker, F. H.
Barr, Archibald, Professor, D.Sc.
Barrie, Charles
Barry, R. E.
Barry, Sir J. Wolfe, K.C.B., F.R.S.
Barter, Benjamin J.
Barth, Daniel N. van Kervel
Bates, Lindon W.
Bayford, J. J.
Beardmore, William
Belam, Henry, Commander, M.V.O., R.N.
Bell, H. S.
Bell, James
Benson, T. B. F.
Beresford, The Right Hon. Lord Charles, G.C.V.O., K.C.B., Admiral
Bethell, The Hon. Alexander E., Rear-Admiral, C.M.G.
Bircham, F. R. S.
Blake, William M.
Blencowe, Frederic John
Blohm, E. G.
Borja de Mozota, A. L. J.
Bostelman, Anatol N.
Bowden-Smith, Sir Nathaniel, K.C.B., Admiral
Bowles, T. Gibson
Box, H. T.
Boyn, C. N.
Brady, H. W.
Brassey, The Right Hon. Lord, G.C.B., D.C.L., LL.D. (*Past President*)
Brassey, The Hon. Thomas A. (*Assoc. Mem. Council*)
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Brims, D. Nicholas
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Brodie, G. W.
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Brown, A. H.
Brown, Joseph
Browne, C. Knightley, Comm. R.N.R.
Bullivant, W. P.
Burdon, A. E.
Burlamagui, Armando, Commander B.N.
Burrell, George

Cail, W.
Caird, J.
Campbell, Sir Charles, Vice-Admiral, K.C.M.G., C.B., D.S.O.
Campbell-Thompson, J.
Canet, G.
Carr, Henry John, Admiral
Carttar, Edward Arundel
Cattori, M.
Cawdor, The Right Hon. Earl (*President*)
Chadburn, A. W.
Chapman, Henry
Chasseloup-Laubat, Marquis de
Chubb, Richard
Clark, John A.
Clark, John
Cleveland, H. F., Admiral
Cohan, E. A.
Cohen, Arthur, K.C., The Right Hon.
Colville, A.
Cook, E. L.
Cookes, T. S.
Cooper, Edward E.
Cooper, H.
Corbu, Jack, B.A.
Corkhill, Thomas W.
Corry, Robert
Cotterill, J. H., Professor, M.A., F.R.S. (*Honorary Vice-President*)
Craddock, T. F.
Craig, James, jun., Captain, R.I.R.
Crease, T. S., Commander, R.N.
Crighton, Alexander T.
Crompton, Frank
Currie, G. W.
Cummins, C. P. C.
Custance, Sir Reginald, K.C.M.G., K.C.B., C.V.O., Admiral

*Da Costa, S. J.
Dalby, W. E., Professor, M.A., B.Sc.
Davis, A. H., Major
Dawson, James, jun.
Deane, Reginald
De Horsey, Sir Algernon F. R., K.C.B., Admiral
Delaunay-Belleville, Louis
Dempster, John

* Since deceased.

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Denny, John M., M.P.
Depree, A. O.
Dickinson, C. F.
Dillon, Malcolm
Dixon, Frank E.
Dixon, James (*Honorary Vice-President*)
Dobbie, J. C.
Dodd, J. B.
Donald, James A.
Donaldson, Thornycroft
Donaldson, William A.
Douglas, Sir Archibald, Admiral, G.C.V.O., K.C.B.
Douglas, J. Tory
Dove, E. J.
Doxford, Robert
Drzewiecki, S.
Duckham, F. E.
Du Dezert, L. Beard, Lieut., French Navy, M.V.O.
Dumaresq, John Saumarez, Commander, R.N.
*Duncan, James
Dunkerley, Stanley, Prof., D.Sc.
Dunlop, Robert J.
Dunning, Arthur G.

Eccles, E.
Eckart, William R., C.E.
Edye, J. Alfred
Eidlitz, Leopold, jun.
Ellis, Charles E. (*Assoc. Mem. Council*)
Ellis, W. H. M.
Epes, H. S.
Erikson, Anthony
Etheridge, C. K.
Etherington, John A. E.
Evans, Thomas L.
Ewing, James A., Professor, C.B., LL.D., F.R.S.

Fairfax, Joseph S.
FitzGerald, C. C. P., Admiral (*Assoc. Mem. Council*)
Fitzgerald, D. W.
Flemmich, G. F.
Foster, J. W.
Foster, M. Henry
Fox, E. Marshall
Fradgley, J. B.
Fraissinet, Alfred
Framjee, S. R.
Free, Thomas, Captain
Fremantle, The Hon. Sir Edmund R., G.C.B., C.M.G., Admiral
Froude, R. E., LL.D., F.R.S. (*Honorary Vice-President*)

Gandhi, S. K.
Garcia, D. C., Captain, Argentine Navy
Garrett, S.
George, Edward J.
Ghirardi, Luigi
Gibbons, John H., Commander, Naval Attaché to the U.S. Embassy
Gilchrist, Percy C.
Glasgow, The Right Hon. the Earl of, G.C.M.G., LL.D. (*Past President*)
Gledhill, J. M.
Glover, Sir John (*Honorary Vice-President*)
Goddard, Frederick D.
Gore, T. H.
Gowan, William
Graham, F.
Graham, The Most Hon. the Marquess of, C.V.O. (*Assoc. Mem. Council*)
Grainger, Thomas L., Captain
Graves, W. T.
Green, W. Miall
Gretton, John, M.P.
Grieve, W.
Gruning, H. H.
Guppy, Thos. R.

Hadfield, Sir Robert A.
Hamilton, George, Lieutenant, R.N.R.
Hamilton, The Right Hon. Lord George, G.C.S.I. (*Honorary Vice-President*)
Harfield, W. Horatio
Harrison, Sewell
Hart, H. V.
Hartley, W. A.
Hartmann, August
Hartmann, George
Hartmann, Wilhelm
Harvey, J. Massey
Hay, James M.
Hay, The Right Hon. Lord John, G.C.B., Admiral of the Fleet (*Honorary Vice-President*)
Hay, The Right Hon. Sir John Dalrymple-, Bart., G.C.B., D.C.L., LL.D., F.R.S., Admiral (*Honorary Vice-President*)
Hay, William R. G.
Head, Christopher
Heckstall-Smith, B.
Hele-Shaw, H. S., LL.D., F.R.S.
Henderson, Francis
Henderson, George T.
Henderson, Wilfred, Commander, R.N.
Henderson, William H., Vice-Admiral
Hills, Arnold F.
Hodges, Petronius
Hodgkinson, George, Captain
Holzapfel, A. C. A.
Holzapfel, A. G.
Holzapfel, Max
Hood, W. H.
Hope, L. C.
Hopkinson, Evelyn S.
Houlder, A. F.
Houlder, M. C.
Howard, J. Eliot
Howard, W. Van Sittart, Commander, R.N.
Hoyle, J. Rossiter
Hume, W. C.
Hunsiker, Millard, Colonel
Hurndall, W. F.
Hutchinson, Henry W.

* Since deceased.

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Inverclyde, The Right Hon. Lord (*Assoc. Mem. Council*)
Isdale, William
Ismay, James H.
Ismay, J. Bruce (*Assoc. Mem. Council*)
Ivanoff, N., Colonel, Imperial Russian Navy

Jack, Thomas
Jackson, Sir Henry B., Rear-Admiral, K.C.V.O., F.R.S. (*Assoc. Mem. Council*)
Jacobi, C. Adolph
James, J. M., Captain
Japp, John
Jaques, W. H., Captain, late United States Navy
Jarman, Stephen, R.N.R.
Jellicoe, Sir John R., Rear-Admiral, C.B., K.C.V.O. (*Assoc. Mem. Council*)
Jenks, W. John, Lieut., R.N.R.
Jesseman, Willie
Johnstone, George F.
Jullien, M.
Julyan, H. E.

Kannegiesser, J.
Keen, Percy
Kelly, Alexander
King, John W.
King, Percy L.
Kirkaldy, John
Kirkaldy, Thomas
Klein, David A., Captain
Knandt, Otto
Knight, C. A.
Knudson, Gunnar
Kriloff, A., Captain, Professor, Imperial Russian Naval Academy
Kröller, A. G.

Lamb, R. O.
Lambert, Carlton J., Professor
Lambton, The Hon. Sir Hedworth, K.C.V.O., K.C.B., Vice-Admiral
Lardner, D. S., Captain
Lasche, Oscar
Latorre, Juan José, Rear-Admiral, Chilian Navy
Launchbury, W.
Laws, George U.
Lawson, W. Robert
Le Doux, Richard
Lees, Edgar, Captain, R.N.
Legge, J. A.
Lewes, Vivian B., Professor, F.I.C., F.C.S. (*Assoc. Mem. Council*)
Leyland, C. J.
Likhatchof, J., Vice-Admiral, Imperial Russian Navy
Lindbeck, Johannes
Linley, A.
Linley, Joseph A.
Lister, W. N., Lieut., R.N.R.
Lloyd, R. Samuel
Long, A. E.
Lund, William
Lunn, John T.
Lyle, R. P.

McAusland, Archibald, Captain
Macarthy, George E.
MacBrayne, Laurence
MacDougall, Dugald
Mace, F. W.
Macfadyen, J. B.
Madden, Charles E., Captain, R.N., C.V.O.
McGeoch, L. A. M.
McGregor, A. Gow
McLaren, Henry Duncan, M.A., M.P.
McLaren, Sir Charles, Bart., M.P., K.C.
McLellan, John
MacIlwraith, A.
Magill, Henry, Captain
Margesson, W. H. D., Commander, R.N.
Marr, James
Martin, E. P.
Martino, The Chevalier Eduardo de
Mechan, H.
Menier, G.
Metcalfe, John
Metcalfe, John George E.
Meyer, Franz J.
Miller, James W.
Millet, Josiah B.
Milne, George
Morant, Sir George Digby, K.C.B., Admiral
Mörch, C. J., Captain, R.N.N.
Morgan-Giles, Frank C.
Mosley, Paget, Colonel
Mulherion, G. F.

Nasservanji, H.
Neal, William G.
Nevill, Henry R., B.A.
Newby, A., Captain
Newman, A. R.
Niven, Sir W. D., K.C.B., F.R.S., D.Sc.
Noakes, G. H.
Noble, Sir Andrew, Bart., K.C.B., F.R.S., LL.D., D.Sc. (*Honorary Vice-President*)
Noel, Sir Gerard H., K.C.B., K.C.M.G., Admiral

Owen, H., Lieut., R.N.R.

Panton, Charles E.
Parfitt, James L., Commander
Park, J. Smith, Colonel, M.V.O., V.D.
Parker, J. Henry
Parsons, P. R.
Pears, Roland
Pearson, Alfred T. D., Captain
Peat, James B.
Percival, J. W.
Petersen, Otto L.
Petersen, William
Petri, Charles
Petrie, David
Pharo, Jacob C.
Pile, Charles H.
Pile, John W.
Pinkney, Thomas

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Pooley, C. A.
Potts, W. Fleming, J.P.
Pratt, G. H. F.
Prendiville, John E.
Preston, F. G. Panizzi
Preston, H. W.
Proctor, John H.
Putnam, Thos.

Raouf, H., Lieut.-Comm., I.O.N.
Ravier, S. Louis
Read, William
Reincke, R. Leopold H.
Renton, A. H.
Renwick, George
Renwick, William Henry
Reynolds, Edward P.
Richardson, John H.
Riley, James (*Assoc. Mem. Council*)
Ritson, Arthur
Roby, Albert W.
Rockett, John E.
Rognetta, F. B., Colonel
Rosenthal, J. H.
Rosse, The Right Hon. the Earl of, K.P., D.C.L., LL.D., F.R.S.
Rothwell, W. H., Captain, R.N.R.
Rouse, Herbert J.
Rowley, Charles J., Admiral
Rugg, C. H.
Runciman, Walter, jun.
Ruthven, J. F., Captain

Sachs, E. O.
Sankey, M. H. P. Rial, Captain, R.E. (retired)
Scantlebury, Vincent J.
Schilling, F., Captain, Naval Attaché to the French Embassy
Schlutow, Albert
Scholefield, A.
Schwanhausser, W.
Scott, Ernest
Scott, Harold H. S.
Scott, J. Henry
Scott, Thomas A.
Seaman, Charles J.
Segrave, T. George
Selborne, the Right Hon. the Earl of, G.C.M.G. (*Honorary Vice-President*)
Semler, Johannes
Serena, Arthur
Seton, Bruce G., Captain
Seymour, Sir Edward Hobart, G.C.B., G.C.V.O., Admiral of the Fleet
Sibbick, A. Edward
Simpson, J. B.
Singer, F. M.
Skoda, Karl von
Sloan, W.
Smith, B. Leigh
Smith, C. Dalrymple
Smith-Carington, H. H.
Smith, Edward
Smith, Henry C.
Smith, T. Knightly
Spence, G. O.
Spencer, George L.
Spencer, Right Hon. Earl, K.G., D.C.L., LL.D. (*Honorary Vice-President*)
Spencer, John W.
Spooner, G. P.
Stephens, R. E.
Stephens, Thomas W.
Stevens, W. Armstrong
Stewart, J. G.
Stewart, T. Cuthbert
Stewart, Walter J. L.
Stewart, Walter L., Commander, R.N.R.
Stileman, F.
Stoker, R. B.
Stokes, Anson P.
Stove, Alfred Edwin
Stow, Ernest Arthur
Sutherland, Sir Thomas, G.C.M.G., (*Assoc. Mem. Council*)
Swan, H. Dawes
Swinburne, M. W.
Swinton, A. A. C.

Tamplin, F. A.
Tamplin, T. Ward
Tawse, James
Taylor, George W.
Taylor, R. S., Captain
Temperley, Joseph
Temperley, J. R.
Thomas, H. Blake
Thompson, O. S.
Thomson, Andrew
Thomson, James W., jun.
Tiedeman, Carl
Todd, J. Stanley
Tomikawa, N.
Tomlinson, F.
Torrey, C. F.
Towers, M. G.
Traill, George
Trollope, F. G.

Uribe, Luis, Rear-Admiral, Chilian Navy
Utley, T.

Van Raalte, J.
Vavasseur, Josiah, C.B.
Vickers, A.
Villarey, Carlo Rey di, Lieut., Naval Attaché, Royal Italian Embassy

Wade, W. S.
Walker, W., Colonel, V.D., J.P.
Wallis, R.
Warneford, J. R. K.
Watkinson, W. H., Professor
Watson, George F.
Watson, H. B.
Watson, Sir William
Watts, Fenwick S.
Wawel, Louis de, Commander, Naval Attaché, I. & R. Austro-Hungarian Embassy
Webster, William K.
Welin, Axel
Westerberg, Axel J.
Westmacott, Percy G. B.
White, J. Bell
Whitehead, Henry

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Widenmann, W., Lieut. Comm., Naval Attaché to the I. & R. German Embassy
Wiegand, H.
Williams, Hugh, Lieut., R.N.R.
Williamson, R. H.
Wilson, A. G.
Wilson, Sir Arthur K., Admiral of the Fleet, V.C., K.C.B., K.C.V.O.
Winsloe, A. L., Rear-Admiral, C.V.O., C.M.G.
Wise, Sir William Lloyd
Woosnam, Arthur
Wordingham, Charles Henry
Wylie, R.

Yamamoto, N.
Younger, Robert L.

Students (122).

Adams, Albert
Aird, Andrew
Artsayooloff, N. C.

Bald, Robert
Barter, Arthur
Bartlett, Colin
Bassett, George A.
Bentley, Thomas H.
Biles, John H., jun.
Bird, James
Bottomley, G. H.
Bowdich, H. J.
Bowes, Thomas D., jun.
Brewster, J. R.
Brooks, J. J.
Bruijn, F. P. de
Bunce, Harry G.
Burgess, L. Richard
Butt, Charles J.

Cannon, Arthur
Carter, Charles M.
Cave-Browne-Cave, Thomas R., Engineer Lieut., R.N.
Child, G. H.
Cole, Alfred W.
Conning, Arthur
Coote, John
Coskery, David A.
Crawford, Archibald
Cruickshank, G. G.
Crump, F. W.

Daniel, P. T.
Davies, J. L.
Dight, Sydney R., Engineer Lieut., R.N.
Dippy, J. W.
Dudding, Bernard P.
Dwelly, Edwin

Edgar, S. G.
Edwards, William G.
Eidlitz, Cornel
Eltis, J. L.
Evans, H. B. Wyn
Ewing, Robert

Ferguson, Mark H.
Fleming, A. L.
Foster, John
Froude, William

Gildner, Harry H.
Good, N. B. N.
Goodall, S. V.
Gray, W. D.
Green, G. Alfred
Green, Reginald T.
Grigg, E.

Hackney, G.
Halket, J. Pitcairn, jun.
Hansford, R. V.
Harding, W. A.
Harvey, G. L.
Hegarty, T. J.
Hickey, Frederick
Hopkins, C. J. W.
Horley, A. E.
Hudson, W. H. F.
Hugman, Walter B.

Innes, John

John, E. Butler
Johnson, E. Blumer
Jones, W. A.

Kent, James L.
Kernan, Alexander
Knowlden, Richard Sydney

Lee, F. M.
Lethbridge, Lionel
Lillicrap, Charles S.
Lipton, John
Lochore, Thomas H.
Longley, A. Edward

Maddox, F. J. W.
Maas, Norman
McLean, K. Hugh
McMillan, Stanley J.
McQueen, James
Mayer, F. L.

Neill, John
Nichols, Edward E.
Noble, W. E.

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Ormston, J. M.

Payne, Stephen
Perring, Harold H., Eng. Lieut., R.N.
Pether, Robert P.
Phillips, H. S.
Powell, Frederick W.
Pratt, F. Gordon
Pugsley, S. G. N.

Riddle, A.
Robb, A. M.
Rogers, F. C.

Sanders, Wilfred G.
Sherriff, James
Silk, Tom H.
Simpson, D. S.
Spanner, Edward F.
Stark, Alexander
Strong, Henry
Studd, W. J. O.
Sutton, Ernest

Thompson, J. C.
Turnbull, Harvard

Uglow, Leonard C.

Wall, Arthur T.
Wallace, William L., jun.
Warwick, Percy W.
Watson, A. W.
Westhorp, R. S.
Whiting, W. R. Gerald, B.A.
Willett, E. V. A.
Williamson, Lawrence C.
Willsher, Frederick A.
Wilson, John
Wilson, John C. M.

Yarrow, Harold E.
Young, Francis

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OBJECTS OF THE INSTITUTION.

The objects of the Institution of Naval Architects—which was established to promote the Improvement of Ships, and of all that specially appertains to them—are comprised under three heads:—

First, the bringing together of those results of experience which so many shipbuilders, marine engineers, naval officers, yachtsmen, and others acquire, independently of each other, in various parts of the country, and which, though almost valueless when unconnected, doubtless tend much to improve our Navies when brought together in the printed Transactions of an Institution.

Secondly, the carrying out, by the collective agency of the Institution, of such experimental and other inquiries as may be deemed essential to the promotion of the science and art of shipbuilding, but are of too great magnitude for private persons to undertake individually.

Thirdly, the examination of new inventions, and the investigation of those professional questions which often arise, and were left undecided before the establishment of this Institution, because no public body to which professional reference could be made then existed.

BYE-LAWS AND REGULATIONS.

CONSTITUTION.

1. The Institution of Naval Architects shall consist of five classes, viz., Honorary Members, Members, Associate-Members, Associates, and Students.

2. *Honorary Members.*—The Class of Honorary Members shall consist of persons upon whom the Council may see fit to confer an honorary distinction.

3. *Members.*—The Class of Members shall comprise persons who on the 25th March, 1904, were on the Register of Members, and Naval Architects or Marine Engineers thereafter elected or transferred into the class of Members under the provisions of Rules 44, 45 and 48.

4. *Associate-Members.*—The Class of Associate-Members shall comprise persons who on the 25th March, 1904, were on the Register of Associate-Members, and Naval Architects and Marine Engineers who, under the provisions of Rules 45 and 48, may be elected into this class.

5. *Associates.*—The Class of Associates shall comprise persons who, under the provisions of Rules 46 and 48, may be elected into this class.

6. *Students.*—The Class of Students shall comprise persons who, under the provisions of Rules 47 and 48, may be elected into this class.

ELECTION AND DUTIES OF OFFICERS.

7. The Officers of the Institution shall consist of a President, Past Presidents, Honorary Vice-Presidents, Vice-Presidents, Members of Council, Associate Members of Council (not exceeding in number one-third the number of Members of Council), a Treasurer, two Auditors of Accounts, and a Secretary or Secretaries.

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8. A General Meeting of the Members, Associate-Members, and Associates of the Institution shall be held annually before Easter in each year; and at this Annual General Meeting the Members of Council, Associate Members of Council, Treasurer, and Auditors for the ensuing year shall be elected.

9. At the Annual General Meeting Members and Associate-Members only shall vote in the election of Members of Council, and both Associates, Members, and Associate-Members in the election of Associate Members of Council, the Treasurer, and the Auditors.

10. *President.*—Both Members and Associates of the Institution shall be eligible for election as President. The President shall preside over all meetings of the Institution, and of Officers of the Institution, at which he is present, and shall regulate and keep order in the proceedings.

11. *Honorary Vice-Presidents.*—Honorary Vice-Presidents shall be Vice-Presidents elected on account of their distinguished position, or Vice-Presidents who have given long and honourable service to the Institution. They shall be elected by the Council at an ordinary meeting thereof, they shall not be subject to re-election, and their numbers shall not be limited.

12. *Vice-Presidents.*—Both Members and Associates of the Institution shall be eligible for election as Vice-Presidents. In the absence of the President, one of the Past Presidents, Honorary Vice-Presidents, or one of the Vice-Presidents shall preside at the General Meetings of the Institution, and shall regulate and keep order in the proceedings.

13. In case of the absence of the President, Past Presidents, and of all the Vice-Presidents, the Meeting may elect any Member of Council or Associate Member of Council, and in case of their absence any Member present to preside.

14. The Chairman, at any Meeting of the Council or of the Institution, when the votes of the Meeting, including his own, are equally divided, shall be entitled to give a casting vote.

15. *Past Presidents, Honorary Vice-Presidents, and Vice-Presidents.*—All Members who have held the posts of President, Honorary Vice-President, or Vice-President shall, while their connection with the Institution as Members lasts, be entitled to sit and vote with the Members of Council.

16. *Members of Council.*—Members only shall be eligible for election as Members of Council at the Annual General Meeting.

17. *Associate Members of Council.*—Associates only shall be eligible for election as Associate Members of Council at the Annual General Meeting.

18. The Direction and Management of the Institution shall be vested in the Council for the time being, the Associate Members of Council voting with the Members of Council in all cases, except in the decision of questions directly affecting the forms of ships and the construction of their hulls.

19. The Council shall meet as often as the business of the Institution requires, and at every Meeting five Members of Council shall form a *quorum*.

20. The Council may appoint Committees to report to them upon special subjects.

21. All questions shall be decided in the Council by vote; but at the desire, expressed in writing, of any four Members or Associate Members of Council present, the determination of any subject shall be postponed to the succeeding meeting of the Council.

22. An annual statement of the funds of the Institution, and of the receipts and payments of the past year, shall be made under the direction of the Council, and, after having been verified and signed by the Auditors, shall be laid before the Annual General Meeting.

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23. The Council shall draw up an Annual Report on the state of the Institution, which shall be read at the Annual General Meeting.

24. It shall be the duty of the Council to adopt every possible means of advancing the Institution, to provide for properly conducting its business in all cases of emergency, such as the death or resignation of Officers, and to arrange for the publication of the Papers read at the Meetings, or of such documents as may be calculated to advance the objects of the Institution.

25. *Treasurer.*—Only Bankers, or Members of Council, or persons who have been Members of Council and are still Members of the Institution, shall be eligible for election as Treasurer.

26. *Trustees.*—There shall be four Trustees, two of whom shall be the President and Treasurer of the Institution for the time being. The remaining two shall be appointed by, and hold office at the pleasure of the Council. In the names of these Trustees, under the direction of the Council of the Institution, all securities shall be taken and investments made, the whole of the property being, notwithstanding, subject to the disposition of the Council, and the order of the Council in writing, signed by the Chairman of the Meeting and countersigned by the Secretary, shall be obligatory upon and full authority for the Trustees.

27. *Auditors.*—All Members, Associate-Members, and Associates of the Institution shall be eligible for election as Auditors.

28. The Auditors shall have access at all reasonable times to the Accounts of the pecuniary transactions of the Institution; and they shall examine and sign the annual statement of the Accounts before it is submitted by the Council to the Annual General Meeting.

29. *Secretary.*—The Secretary or Secretaries shall be elected by the Council, and shall be removable at the will of the Council, after due notice given. The salary of the Secretary or Secretaries shall be fixed by the Council.

30. It shall be the duty of the Secretary, under the direction of the Council, to conduct the correspondence of the Institution; to attend all Meetings of the Institution and of the Council; to take Minutes of the proceedings of such Meetings; to read the Minutes of the preceding Meeting; to announce donations made to the Institution; to superintend the publication of such Papers as the Council may direct; to have charge of the library, museum, and offices of the Institution; and to direct the collection of subscriptions and the preparation of accounts. He shall also engage, and be responsible for, all persons employed under him, and generally conduct the ordinary business of the Institution.

ELECTION OF COUNCIL.

31. In each year nine Ordinary and three Associate Members of Council shall retire, unless before the date of drawing up the Balloting Lists for the election of the Council any Members of the Council shall have died or resigned, in which case only so many Members shall retire as shall be necessary in order to make up the number to nine Ordinary and three Associate Members of Council, subject always to the provisions of Rule 37. The Members who shall retire in each year shall be those who have served longest on the Council from the date of the last election, and in the event of there being several Members who have served an equal time on the Council, the order of retirement amongst these shall be alphabetical. The retiring Members shall be eligible for re-election.

32. In January of each year the Council shall meet and prepare Lists for the election of the Council for the ensuing year. These Lists shall be as follows, namely:—

1*st*. A List of the names of the President, Vice-Presidents, and Treasurer for the ensuing year to be submitted at the Annual General Meeting, for their election in a body.

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2*nd*. A list of candidates to fill any vacancies in the list of Vice-Presidents.

3*rd*. Lists for the election of the Ordinary Members and Associate Members of Council.

33. No addition shall be made to the lists of Vice-Presidents until, by death or resignation, their number shall have been reduced to below sixteen, after which their numbers shall be raised to and preserved at sixteen.

34. When any vacancy occurs in the list of Vice-Presidents which it is intended to fill, the election shall be by voting papers issued to all Members and Associate-Members of the Institution. The candidates to fill the vacancy shall be selected by the Council in the month of January from among the existing or past Members of Council. The number of candidates shall be double the number of vacancies to be filled, but any Member of the Institution shall be at liberty to substitute for the name or names on the Balloting List any other name or names he may prefer. The voting papers shall be issued to the Members and Associate-Members at the same time as the voting papers for the election of Members of Council, and shall be subject to the same regulations and scrutiny as these latter, as provided for by Rules 37, 38, 39, 40 and 41.

35. After having been once elected by voting papers, the Vice-Presidents will be subject to re-election every year in a body at the Annual Meetings.

36. The total number of Ordinary Members of Council shall be thirty-six, and that of Associate Members of Council twelve. These numbers shall be exclusive of the President, Honorary Vice-Presidents, Vice-Presidents, and Treasurer. If in any year the numbers on the Council fall below those prescribed above, the number of vacancies provided by Rule 31 shall, for that year, be increased by a number not exceeding five in the case of Members of Council, and not exceeding three in the case of Associate Members of Council.

37. At or before the date of issuing the Syllabus of the Annual General Meetings in each year, the Lists proposed by the Council for the election of Members to fill the vacancies in the Council for the ensuing year shall be printed, and sent to all Members and Associate-Members to serve as Balloting Lists. These Lists shall, irrespective of any vacancies there may be in the list of Vice-Presidents, contain, first, the names of the retiring Ordinary Members of Council at the time of the preparation of the Balloting Lists, together with at least nine new names of Members of the Institution, and the whole of these names shall be printed in alphabetical order. Secondly, the names of the retiring Associate Members of Council at the time of the preparation of the Balloting Lists, together with at least three new names of Associates of the Institution, and these names shall also be printed in alphabetical order. From these Lists the vacancies in the Council shall be filled up. Any vacancies in the list of Members of Council or Associate Members of Council beyond those existing at the time of the issue of the Balloting Lists that may be created by the election of Honorary Vice-Presidents or Vice-Presidents shall, upon the return of the Scrutineers' Lists, be filled by the names of the candidates next in order who have obtained the greatest number of votes, subject to the conditions of the ballot. Every Member and Associate-Member shall vote for as many names on each of the Lists as there are vacancies to be filled, but not for more, and shall be at liberty to substitute for any name or names on the Balloting List any other name or names he may prefer.

38. A similar Balloting List (in which, however, the names of the Ordinary Members of Council proposed for election shall not be included) shall be printed and sent to all Associates of the Institution, to serve as a Balloting List for Associates, from which the voting for Associate Members of Council shall be taken. Every Associate shall vote for as many names on that List as there are vacancies to be filled, but not for more, and shall be at liberty to substitute for any name or names on the Balloting List any other name or names he may prefer.

39. The Balloting Lists may be sent by post or otherwise to the Secretary, and must reach him at least one clear week before the day named for the Annual General Meeting. They are to be handed by the

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Secretary to the Scrutineers appointed at the preceding Annual General Meeting, who shall verify the lists and count the votes. No Balloting List shall be considered valid by the Scrutineers that contains either more or fewer names than there are vacancies to be filled. The Scrutineers shall prepare a list of the names which have obtained the greatest number of votes subject to the conditions of the Ballot. This list shall be placed in a sealed envelope by the Scrutineers and handed to the Secretary, who shall present it to the Chairman of the Annual General Meeting.

40. At the opening of the Annual General Meeting the order of business shall be :—

(1) To read and consider the Reports of the Council and Treasurer.

(2) To read the List of Officers and Nomination for Council for the ensuing year, proposed by the Council.

(3) The Chairman shall next put to the Meeting the List containing the names of the President, Vice-Presidents, and Treasurer for election for the ensuing year.

(4) The Chairman shall nominate the Scrutineers, who shall be charged with the examination of the Balloting Lists which will be received prior to the next Annual General Meetings. The number of these Scrutineers shall be determined by the Chairman, but shall not be less than four, one of whom shall be a member of the existing or proposed Council.

(5) The Chairman shall then read the List presented by the Scrutineers, and shall declare the gentlemen named in the List to be duly elected, provided always that the List does not contain more names than there are vacancies to be filled. If, in consequence of two or more of the candidates receiving an equal number of votes, the List shall contain more names than there are vacancies, the Council shall, at their next meeting, decide which of these candidates shall be elected.

(6) The Meeting will proceed to the other business before it.

41. The new Council and Officers shall take office immediately after the close of the Annual General Meeting.

42. In the event of any vacancy occurring in the offices of either President or Treasurer after the date of the Annual Election in any year, the Council shall have power to elect a new President or Treasurer as the case may be, who shall hold office till the conclusion of the next Annual General (Spring) Meeting of the Institution.

DESIGNATION AND ELECTION OF MEMBERS, ASSOCIATE-MEMBERS, ASSOCIATES, AND STUDENTS.

43. Any Honorary Member, Member, Associate-Member, Associate, or Student, having occasion to designate himself as belonging to the Institution, shall state the class to which he belongs according to the following abbreviated forms, viz., Hon. Mem. Inst.N.A.; M.Inst.N.A.; A.M.Inst.N.A.; Associate Inst.N.A., Stud. Inst.N.A.

44. *Admission of Members.*—Every Candidate for election, or for transfer, into the Class of Members shall, at the time of such election or transfer, be more than thirty years of age, and shall comply with the regulations set forth below in Rule 45, stating the qualifications required of Candidates for Associate-Membership, and shall further show to the satisfaction of the Council that he has occupied a position of primary responsibility for a period of at least five years.

45. *Admission of Associate-Members.*—Every Candidate for election, or for transfer, into the class of Associate-Members shall, at the time of such election or transfer, be more than twenty-five years of age, and shall comply with regulations (*a*) to (*c*) below :—

He shall submit a statement showing to the satisfaction of the Council that he has—

(*a*) Served an apprenticeship, or pupilage, in shipbuilding or marine engineering for at least five years, or

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(*b*) Undergone a mixed training of at least four years, partly in shipbuilding or marine engine works, and partly in a school of Naval Architecture or Marine Engineering recognised by the Council of the Institution.

He shall further show that, after completing his training, as set forth under headings (*a*) or (*b*), he has been professionally employed in some public or private shipbuilding yard, or marine engine works, or technical registration society, or as professional superintendent of a steamship company, and shall give full particulars of the nature and duration of such employment.

In the event of the Candidate not having complied with the conditions under headings (*a*) or (*b*), he shall show—

(*c*) That he has been regularly educated by apprenticeship, or pupilage, or in a technical school, as a civil or mechanical engineer, and that he has been subsequently *professionally* employed for at least seven years in some public or private shipbuilding yard or marine engine works.

This statement shall be signed by at least three Members, or Associate-Members, of the Institution, whose signatures shall certify their personal knowledge of the Candidate and agreement with his statement.

46. *Admission of Associates.*—Every Candidate for election into the class of Associates shall, at the time of such election, be more than twenty-five years of age, and shall be qualified either by profession or occupation, or by scientific or other attainments, to discuss with naval architects the qualities of a ship, or the construction, manufacture, or arrangement of some part or parts of a ship or her equipment. He shall submit a statement showing to the satisfaction of the Council his claims to be admitted as an Associate. This statement shall be signed by at least one Member, Associate-Member, or Associate of the Institution.

47. *Admission of Students.*—Every Candidate for election into the class of Students shall, at the time of such election, be more than eighteen and less than twenty-five years of age, and shall submit a statement showing, to the satisfaction of the Council, that he is either an apprentice, an articled pupil, or a student, in naval architecture or marine engineering. This statement shall be signed by at least two Members, or Associate-Members of the Institution, whose signature shall certify their personal knowledge of the Candidate and agreement with his statement. Before attaining the age of twenty-six years, Students shall apply for transfer to the class of Associate-Members or Associates, if they desire to remain connected with the Institution. They cannot continue to be Students after attaining the age of twenty-six years.

48. *Election.*—The recommendation of the Council with respect to the application of a Candidate for election to the Institution shall be submitted to the Members, Associate-Members, and Associates (who shall have access to the applicant's statement) at an ordinary meeting of the Institution for them jointly to vote upon; the voting to be by ballot, should a ballot be demanded. Associates shall be eligible to vote only upon the proposal of a Candidate for Associateship.

The proportion of votes for deciding the election of Members, Associate-Members, Associates, and Students shall be at least four-fifths of the numbers recorded.

49. *Examination.*—The Council may, from time to time, cause examinations to be held, to which anyone desirous of so doing may present himself after complying with such regulations as the Council may prescribe. On the result of such examinations a certificate or diploma of competency in naval architecture or marine engineering will be issued to successful candidates, and this certificate (in the case of Candidates who are not already members of the Institution) may, if coupled with sufficient practical experience, be accepted by the Council as qualification for admission to the Institution. The fees payable by the Candidates for such examination shall from time to time be determined by the Council.

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SUBSCRIPTIONS.

50. *Subscriptions.*—Each Member, Associate-Member, and Associate shall pay an entrance fee of two guineas, and an annual subscription of two guineas in advance; each Student shall pay an annual subscription of fifteen shillings in advance, and shall, on his transfer to the class of Associate-Members or Associates, pay an entrance fee of one guinea, provided such transfer takes place within one year of his ceasing to be a Student. The first subscription shall be payable on election, and all future subscriptions on the 1st day of January of each year. Any Member, Associate-Member, Associate or Student, withdrawing from the Institution after the first day of January of any year shall pay the subscription due on that day.

51. Any Member, Associate-Member, or Associate may compound for his annual subscription, for life, by a single payment of not less than thirty guineas.

52. No person's name shall be entered on the Roll as Member, Associate-Member, Student, or Associate of the Institution nor possess the privileges of Membership (except it be on the honorary list) until he shall have paid his first subscription or the life composition, and if the payment be delayed for more than twelve months from the date of his election, the same shall be void unless the Council otherwise direct.

53. The Secretary shall at the close of every year notify to all Members, Associate-Members, Students, and Associates whose subscriptions for that year shall not have been paid, that it will be his duty to report accordingly to the Council, and he shall at the same time furnish the person whose subscription is in arrear with copies of this and of the two following Rules.

54. The Secretary shall before Easter in every year lay before the Council a list of all Members, Associate-Members, Students, and Associates whose subscriptions for the two previous years shall be still unpaid, and unless the Council shall otherwise direct, the names of those in arrear shall be expunged from the Roll of Members, Associate-Members, Students, and Associates, and shall not be replaced without re-election in due form. Provided always that the Council shall at any time within two years therefrom have power to dispense with such re-election, and to restore the name to the Roll upon payment of all subscriptions then due, and upon cause being shown to the satisfaction of the Council why such subscriptions were not previously paid.

55. Nothing herein contained shall prejudice the right of the Institution to the legal recovering of all arrears of subscriptions up to the date of striking the name off the Roll.

56. In case the Council should be of opinion that any Member, who has been long distinguished in his professional career, from ill-health, advanced age, or other sufficient causes, should not be called upon to continue his annual subscription, they may remit it. Also they may remit any arrears which are due from an individual, or may accept a collection of books, or drawings, or models, or such other contribution as, in their opinion, under the circumstances of the case, may entitle the person to be enrolled as a Life Subscriber, or to enable him to resume his former rank in the Institution which may have been in abeyance from any particular causes. These cases must be considered and reported upon by a Sub-Committee named for the purpose.

EXPULSION OF MEMBERS.

57. In case the expulsion of any individual shall be judged expedient by ten or more Members, and they think fit to draw up and sign a proposal requiring such expulsion, the same being delivered to the Secretary shall be by him laid before the Council. If the Council, after due inquiry, do not find reason to concur in the proposal, no entry thereof shall be made in any Minutes, nor shall any public discussion thereon be permitted; but if the Council do find good reason for the proposed expulsion they shall direct the Secretary to address a letter to the person proposed to be expelled, advising him to withdraw from the Institution. If that advice be followed, no entry on the Minutes nor any public discussion on the subject

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shall be permitted; but if that advice be not followed, nor a satisfactory explanation given, the Council shall call a Special General Meeting of Members, Associate-Members, and Associates, for the purpose of deciding on the question of expulsion; and if two-thirds of the persons present at such Special General Meeting, provided the number so present be not less than thirty, vote that such individual be expelled, the Chairman of that Meeting shall declare such expulsion accordingly, and the Secretary shall communicate the same to the individual.

MEETINGS.

58. Meetings for the Reading of Papers shall be held as frequently, and at such times, as the Council may determine.

TRANSACTIONS.

59. The *Transactions* of the Institution, including the Papers read at the Ordinary Meetings, and Reports of the Discussions by which they are followed, shall be edited by the Secretary, and printed under the direction of the Council.

60. A copy of each Volume of *Transactions* shall be sent free to every Member, Associate-Member, Student, and Associate.

61. The Secretary, under the direction of the Council, may dispose of the surplus stock of *Transactions* which have been published more than three years, at a price to be determined from time to time by the Council, provided a sufficient number remain on hand to supply the probable demand of new Members, Associate-Members, Students, and Associates, to complete their sets by the purchase of the back Volumes.

CHANGE OF ADDRESS.

62. Members, Associate-Members, Students, and Associates are particularly requested to communicate to the Secretary any change of address.

ALTERATION OF RULES.

63. No rule shall be altered or rescinded and no new rules shall be made save by the consent of a two-thirds majority of Members, Associate-Members, and Associates voting at the Annual General Meeting of the Institution or at a Special General Meeting of the same called together for the purpose, provided the number there present be not less than twenty. And the proposed alterations in, additions to or rescission of any rule shall, together with the notice convening such Meeting, whether Special or otherwise, be circulated among all Members, Associate-Members, and Associates of the Institution at least fourteen days before the date of such Meeting.

VOTING AT GENERAL MEETINGS.

64. Every question submitted to a General Meeting, whether Ordinary or Special, shall be decided in the first instance by a show of hands, and in every case of a question to be decided by an equality of votes the Chairman shall, both on a show of hands and at a ballot, have a casting vote in addition to the vote to which he may be entitled as a Member. If a ballot is demanded, it shall be taken in such manner, at such place, and either immediately or at such other time thereafter as the Chairman shall before the conclusion of the Meeting direct, and the result of such ballot shall be deemed to be the resolution of the Institution in such General Meeting. The demand for a ballot shall not prevent the continuance of a Meeting for the transaction of any business other than the question on which a ballot has been demanded

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SPRING MEETINGS OF THE FORTY-NINTH SESSION

OF THE

INSTITUTION OF NAVAL ARCHITECTS.

APRIL 8, 9, AND 10, 1908.

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INTRODUCTORY PROCEEDINGS.

THE Spring Meetings of this, the Forty-ninth Session of the Institution of Naval Architects, were held on April 8, 9 and 10, in the Hall of the Society of Arts, John-street, Adelphi, W.C.

The opening meeting was presided over by the Right Hon. the Earl of Glasgow, G.C.M.G., LL.D., President of the Institution, who commenced the proceedings by calling on the Secretary, Mr. R. W. Dana, to read the Report of Council, which was as follows :—

ANNUAL REPORT OF THE COUNCIL FOR 1908.

The Council have pleasure in submitting their annual report upon the progress made during the past year. The total number of new candidates elected during that period was 137; but, owing chiefly to the numerous losses by death, the net gain in membership is reduced to 38, the actual numbers on the books being given below.*

The Treasurer's report shows a satisfactory financial statement for the past year.

The Council have received with regret the resignation of the Earl of Glasgow, G.C.M.G., from the office of President, which he has occupied with so much distinction and advantage to the Institution since 1901. The Council desire to express their cordial thanks to Lord Glasgow for his services during his period of office, and their high appreciation of the courtesy and tact which his lordship has invariably displayed while presiding over the meetings of the Council and of the Institution.

A Summer Meeting was held in Bordeaux last June jointly with the French, German, and American Societies of Naval Architects. The meeting constituted an International Congress of Naval Architecture, under the joint Presidency of Lord Glasgow and Monsieur Bertin, and was attended by delegates from the principal maritime countries. Owing to the large number of papers contributed, it was only found possible to include in the Transactions of each Institution those papers which were read by its own members. The reception accorded by the local authorities and the influential residents was most cordial; and the Maritime Exhibition, with its valuable collection of British ship models, formed an interesting feature of the meeting. Visits were paid to the various shipbuilding and engineering works in the neighbourhood, notably to the Chantiers de la Gironde, Messrs. Dyle & Bacalan, and also to the famous vineyards of Château Margaux. Mention must also be made of the subsequent journey by special train to the great steel and armour-plate works of Messrs. Schneider & Co., at Le Creusot, where a thorough inspection of the works was made, and a most hospitable welcome was acccorded to those members of the Congress who were able to make the trip.

The Council have, under Rule XI., elected as Honorary Vice-Presidents Dr. F. Elgar, F.R.S. Treasurer of the Institution, and Mr. James Dixon, Chairman of Lloyd's Register Society.

* MEMBERSHIP.

	1907.	1906.	1905.
Honorary Members	12	12	13
Members	1,097	1,091	1,070
Associate-Members	118	88	87
Associates	476	495	499
Students	93	72	42
Total	1,796	1,758	1,711

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The Institution of Naval Architects Scholarship, which was competed for last year, has been awarded to Mr. A. M. Robb, of Messrs. G. L. Watson & Co., Glasgow, on the result of the examinations held by the Board of Education in the prescribed subjects, and the successful candidate is now pursuing his studies at Glasgow University.

The Council have received from Mr. A. F. Yarrow, Vice-President, an offer to defray the cost (up to £20,000) of an experimental tank for research purposes to be erected at the National Physical Laboratory, Bushy, provided the expenses of maintenance for the first ten years are assured; and it is suggested that the shipbuilders and shipowners of the country should be invited to guarantee the necessary amount for that purpose. Mr. Yarrow's munificent offer has been gratefully accepted subject to the conditions under which it is made, and a Committee will be formed in order to take such steps as may be necessary to carry the scheme to a successful issue.

The losses through death during the past year have unfortunately been numerous. The Marquess of Linlithgow, who, as Lord Hopetoun, will always be remembered with esteem and affection by the Council and Members of the Institution, passed away last month at the early age of 47. As President from 1896 to 1900, Lord Linlithgow contributed largely to the success of the Institution during a period of particular interest in its history, and his death in the prime of life is one that has caused widespread sorrow among all those whose privilege it had been to appreciate his lordship's great natural gifts for high office, and the charm of manner which was one of his unfailing characteristics. Lord Kelvin, an Honorary Member of this Institution of over thirty years' standing, passed away at the close of last year, mourned by the entire scientific world. The Council also regret to record the death of Mr. J. McFarlane Gray, Vice-President, many of whose valuable contributions to the elucidation of problems in Naval Architecture and marine engineering have appeared in the Transactions; of Mr. Robert Thompson, of Sunderland, and Col. H. F. Swan, C.B., of Newcastle-on-Tyne, Members of Council; and Mr. C. H. Haswell, the oldest and one of the most respected of the marine engineers in the United States.

Among the Associates, the Institution has lost Sir William Pearce, Bart., late Chairman of the Fairfield Shipbuilding and Engineering Company, Sir J. D. Milburn, Bart., and Major-General Sir John Crease, K.C.B., while the death of Mr. John Corry, Honorary Vice-President, has deprived the Council of an old and valued colleague.

The Council have pleasure in announcing that they have awarded an Institution Premium to Mr. Lyonel Clark for his paper on "Modern Floating Docks"; and another to Mr. J Hamilton Gibson for his paper on "Torque of Propeller Shafting: Some Investigations and Results."

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STATEMENT OF RECEIPTS AND PAYMENTS FOR THE YEAR 1907.

Dr. GENERAL ACCOUNT. Cr.

Receipts.	£ s. d.	£ s. d.	Payments.	£ s. d.	£ s. d.
To Balance at Banker's, Dec. 31, 1906	19 9 0		By Rent of Rooms	300 0 0	
,, Balance in Secretary's hands, Dec. 31, 1906	41 14 4		,, Heating and Lighting	9 3 7	
		61 3 4	,, Insurances	4 2 1	
,, Annual Subscriptions		3,403 0 7	,, Cleaning	52 14 0	
,, Life Subscriptions		31 10 0			365 19 8
,, Admiralty Grant		250 0 0	,, Salaries		1,293 0 1
,, Sale of Volumes		229 15 5	,, Printing Account, including Vol. XLIX.	948 17 9	
			,, Despatch of Volumes	101 3 7	
					1,050 1 4
			,, Stationery...	36 12 1	
			,, Postages and Telegrams... ...	69 12 5	
					106 4 6
			,, Telegraphic address	1 1 0	
			,, Telephone	11 11 11	
					12 12 11
			,, Expenses of Meetings (after deducting amount realised by Sale of Dinner Tickets) ...	54 17 7	
			,, Travelling	55 3 8	
			,, Reporting	51 11 6	
			,, Gold Medal	12 10 0	
					174 2 9
			,, Bank Charges	1 19 3	
			,, Auditor's Fee	15 15 0	
			,, Petty Disbursements	32 12 11	
					50 7 2
			,, Investments		758 10 9
			,, Balance at Banker's Dec. 31,1907	134 9 0	
			,, Balance in Secretary's hands, Dec. 31, 1907	30 1 2	
					164 10 2
	£3,975 9 4				£3,975 9 4

Dr. LIBRARY FUND. Cr.

Receipts.	£ s. d.	Payments.	£ s. d.
To Balance at Banker's, Dec. 31, 1906	8 11 4	By Library Expenses	16 4 0
,, Entrance Fees	231 0 0	,, Furniture and Repairs	6 15 6
,, Interest on Investments	435 7 0	,, Investments	600 0 0
		,, Balance at Banker's, Dec. 31, 1907	51 18 10
	£674 18 4		£674 18 4

Dr. MARTELL SCHOLARSHIP FUND, 1907. Cr.

Receipts.	£ s. d.	Payments.	£ s. d.
To Balance at Banker's, Dec. 31, 1906	36 16 2	By Payment to Mr. A. T. Wall	25 0 0
,, Interest on investments	46 6 2	,, Balance at Banker's, Dec. 31, 1907	60 11 2
,, Return of income tax	2 8 10		
	£85 11 2		£85 11 2

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Statement of Receipts and Payments for the Year 1907 (*continued*).

Dr. INSTITUTION OF NAVAL ARCHITECTS SCHOLARSHIP FUND, 1907. Cr.

Receipts.	£	s.	d.	Payments.	£	s.	d.
To Balance at Banker's, Dec. 31, 1906	601	17	5	By Payment to Mr. A. Cannon	50	0	0
,, Interest on deposit	17	18	4	,, Payment to Mr. A. Robb	12	10	0
				,, Advertising	9	1	5
				,, Balance at Banker's, Dec. 31, 1907	548	4	4
	£619	15	9		£619	15	9

FRANCIS ELGAR, *Treasurer.*
R. W. DANA, *Secretary.*

We have examined the above-written entries with the books and vouchers, and find them correct.

BAILL, BAKER, DEED, CORNISH & Co.,
Chartered Accountants,
1, *Gresham Buildings, E.C.*

February 25, 1908.

DONATIONS TO THE INSTITUTION LIBRARY.

The following is a List of Donations to the Library:—

"Annual Report of the Royal National Lifeboat Institution," 1907. *Presented by the Royal National Lifeboat Institution.*

"British Engineering Standards Coded Lists," Vol. V. *Presented by the Publishers,* Messrs. Robert Atkinson, Ltd.

"British Corporation Register for the Survey and Registry of Shipping." 1908. *Presented by the Committee of the British Corporation for the Survey and Registry of Shipping.*

"Bureau Veritas International Register of Shipping," 1908. *Presented by the Council of the Bureau Veritas International Register of Shipping.*

"Bulletin de l'Association Technique Maritime," 1907. *Presented by l'Association Technique Maritime.*

"Bulletin de la Société Scientifique et Industrielle de Marseille," 1907. *Presented by the Editor.*

"Bulletin de la Société d'Encouragement pour l'Industrie Nationale," 1907. *Presented by the Société d'Encouragement pour l'Industrie Nationale.*

"Calendar of the Glasgow and West of Scotland Technical College," 1907-8. *Presented by the Glasgow and West of Scotland Technical College.*

"Cunard Express Liner *Mauretania.*" *Presented by* Messrs. Swan, Hunter & Wigham Richardson, Ltd.

"Cunard Turbine-Driven Quadruple Atlantic Liner *Lusitania.*" *Presented by the* Directors of Messrs. John Brown & Co., Ltd.

"Directory of Shipowners and Shipbuilders and Marine Engineers," 1908. *Presented by* S. R. Blundstone, Esq.

"Engineering Association of New South Wales," Vols. XV., XVI., XVII., XVIII., XXII. *Presented by the Engineering Association of New South Wales.*

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"Engineering Standards Committee. Report of Progress of Work," 1907. *Presented by the Committee.*

"Flottes de Combat, en 1908." By Commandant DE BALINCOURT. *Presented by the Editors*, Messrs. BERGER, LEVRAULT & CIE.

"Germanischer Lloyd: International Register for the Year 1908." *Presented by the Committee of the Germanischer Lloyd.*

"Hydraulics." Vols. I. and II. *Presented by the Author*, Professor S. DUNKERLEY, D.Sc.

"Jahrbuch der Schiffbautechnischen Gesellschaft," 1908. *Presented by the Schiffbautechnische Gesellschaft.*

"Journal of the American Society of Naval Engineers," Vol. XIX., 1907. *Presented by the Society.*

"Journal of the Franklin Institute," 1907. *Presented by the Franklin Institute.*

"Journal of the Iron and Steel Institute," 1907. *Presented by the Iron and Steel Institute.*

"Journal of the Royal United Service Institution," 1907. *Presented by the Royal United Service Institution.*

"Journal of the Royal Society of Arts," 1907. *Presented by the Royal Society of Arts.*

"Journal of the United States Artillery," 1907. *Presented by the Editor of the Journal.*

"Journal of the Western Society of Engineers," 1907. *Presented by the Western Society of Engineers.*

"Königliche Versuchsanstalt für Wasserbau und Schiffbau in Berlin." *Presented by* Herr REGIERUNGS- UND BAU-RAT THIELE.

"Lloyd's Register of British and Foreign Shipping," 1907. *Presented by the Committee of Lloyd's Register.*

"Lloyd's Register of Yachts," 1907. *Presented by the Committee of Lloyd's Register.*

"Mémoires et Comptes-Rendu des Travaux de la Société des Ingénieurs Civils de France," 1907. *Presented by the Société des Ingénieurs Civils de France.*

"Mémorial du Génie-Maritime," 1907. *Presented by the Ministère de la Marine, Paris.*

"Minutes of the Proceedings of the Institution of Civil Engineers," Vols. CLXVII., CLXVIII., CLXIX., CLXX. *Presented by the Institution of Civil Engineers.*

"Modern Steam Turbines." *Presented by the Author*, HERR M. DIETRIECH.

"National Physical Laboratory: Collected Researches," Vol. II. *Presented by the National Physical Laboratory.*

"Naval Annual," 1908. By the Right Hon. Lord BRASSEY, K.C.B., and others. *Presented by the Editors.* JOHN LEYLAND, Esq., and the Hon. T. A. BRASSEY.

"Philosophical Transactions of the Royal Society of London," Vol. CCVII., "A" Series. *Presented by the Royal Society of London.*

"Proceedings of the Institution of Mechanical Engineers," 1907. *Presented by the Institution of Mechanical Engineers.*

"Proceedings of the London Mathematical Society," 1907. *Presented by the London Mathematical Society.*

"Proceedings of the Royal Society of London," 1907. A. B. Series. *Presented by the Royal Society of London.*

"Proceedings of the United States Naval Institute," 1907. *Presented by the U.S. Naval Institute.*

"Report of the British Association for the Advancement of Science," 1907. *Presented by* F. ELGAR, Esq., LL.D., F.R.S.

"Report of the National Physical Laboratory" for the year 1906. *Presented by the Director of the National Physical Laboratory.*

"Report of the Secretary of the Navy (U.S.A.)," 1906. *Presented by the U.S. Navy Department.*

"Report of Tests of Metals and other Materials made at Watertown Arsenal," for the year ending June 30, 1906. *Presented by the Chief of Ordnance, U.S.A.*

"Revue Maritime, 1907." *Presented by the Ministère de la Marine, Paris.*

"Rivista Marittima," 1907. *Presented by the Ministèro della Marina, Rome.*

"Royal Society's Year Book," 1908. *Presented by the Royal Society.*

"Scientific Proceedings of the Royal Society of Dublin," 1907. *Presented by the Royal Society of Dublin.*

"Transactions of the American Institute of Mining Engineers," Vol. XXXVII, 1907. *Presented by the American Institute of Mining Engineers.*

"Transactions of the American Society of Civil Engineers," Vols. LVIII. and LIX., 1907. *Presented by the American Society of Civil Engineers.*

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"Transactions of the American Society of Mechanical Engineers," Vol. XXVIII., 1907. *Presented by the American Society of Mechanical Engineers.*

"Transactions of the Engineering Conference," Sections I., IV., V., VII. *Presented by the Institution of Civil Engineers.*

"Transactions of the Institution of Engineers and Shipbuilders in Scotland," Vol. L. *Presented by the Institution of Engineers and Shipbuilders in Scotland.*

"Transactions of the Junior Institution of Engineers," Vol. XVII., 1907. *Presented by the Junior Institution of Engineers.*

"Transactions of the Institute of Marine Engineers," Vol. XVIII., 1907. *Presented by the Institute of Marine Engineers.*

"Transactions of the Liverpool Engineering Society," 1907. *Presented by the Liverpool Engineering Society.*

"Transactions of the North-East Coast Institution of Engineers and Shipbuilders," Vol. XXIII., 1906-7. *Presented by the North-East Coast Institution of Engineers and Shipbuilders.*

"Transactions of the North of England Institute of Mining and Mechanical Engineers," 1907. *Presented by the North of England Institute of Mining and Mechanical Engineers.*

"Transactions of the Society of Engineers," 1906. *Presented by the Society of Engineers.*

"Transactions of the Society of Naval Architects and Marine Engineers," Vol. XV., 1907. *Presented by the Society of Naval Architects and Marine Engineers.*

"Turbine Vessels" (in Russian). *Presented by the Author*, Mons. A. BORMANN.

"Yacht Racing Association: Rules and Time Allowances," 1908. *Presented by the Yacht Racing Association.*

Newspapers and Magazines, for 1907, presented by their respective Proprietors :—

"African Engineering."
"American Marine Engineer."
"Annalen für Gewerbe und Bauwesen."
"Arms and Explosives."
"Cassier's Magazine."
"Engineer."
"Engineering."
"Engineering Times."
"English Mechanic."
"Field."
"Iron and Coal Trades Review."
"International Marine Engineer."
"Machinery Market."
"Marine Engineer."
"Marine Review."
"Mariner."
"Mechanical Engineer."
"Metal Industry."
"Page's Weekly."
"Royal Automobile Club Journal."
"Saturday Review."
"Science Abstracts."
"Shipping and Mercantile Gazette."
"Shipping World."
"South African Engineering."
"Steamship."
"Syren and Shipping," Illustrated.
"Zeitschrift des Vereins Deutscher Ingenieure."

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The following gentlemen, having been duly recommended by the Council, were unanimously elected :

Members.

ALMEIDA, A. A. ROSAURO DE, Chief Constructor to the Brazilian Naval Commission, *Newcastle-on-Tyne.*
ANDREW, JOHN INGRAM, General Manager to Messrs. G. Fenwick & Co., *Hong Kong.*
AVERY, THOMAS, Constructor, Royal Indian Marine Dockyard, *Calcutta, India.*
*BARBER, W. D., Designer to Messrs. Boulton & Paul, *Norwich.*
BEARBLOCK, CHARLES W. J., Eng.-Comm., R.N., First Assistant to Chief Engineer H.M. Dockyard, *Chatham.*
BERTRAND, ALBERT, Ingénieur Principal de la Marine, Arsenal de Rochefort, *France.*
BLACK, JOHN, Assistant Naval Architect to Messrs. John Brown & Co., *Clydebank.*
BLACK, ROBERT W., Manager, The New Amoy Dock Co., Ltd., *Amoy, China.*
*BUTTERFIELD, A. E., Engineer to the Humber Conservancy, *Hull.*
CAPPS, W. L., Rear-Admiral, Chief of the Bureau of Construction and Repairs, U.S. Navy, *Washington, U.S.A.*
*CHAMBERS, JOHN, Shipbuilder, *Lowestoft.*
COULL, A. B., Consulting Engineer and Marine Surveyor, *Newcastle-on-Tyne.*
CRICHTON, W. H., Engineer-Lieutenant, R.N., of H.M.S. *Pegasus, Australian Station.*
CROISDALE, L. R., Engineer-Lieutenant, R.N., Controller's Department, Admiralty, *Whitehall.*
CUMING, G., Managing Director of Messrs. Harland & Wolff, Ltd., *Belfast.*
DIXON, R. B., Engineer-Commander, R.N., H.M.S. *Dominion, Channel Fleet.*
DUNLOP, JOHN, Chief Draughtsman, Messrs. John Reid & Co., *Glasgow.*
EDWARDS, F., Engineer-Commander, R.N., Engineer Inspector, *Admiralty.*
EMDIN, A. R., Engineer-Commander, R.N., H.M. Dockyard, *Devonport.*
*EVANS, M. T., Consulting Engineer, *Bristol.*
FALCON, JOHN, Superintending Engineer, New Zealand Shipping Co., *London.*
FERRATI, F., Colonel R.I.N., Royal Arsenal, *Spezia, Italy.*
GIBSON, J. HAMILTON, Assistant Engineering Manager, Messrs. Cammell, Laird & Co., *Birkenhead.*
GOODWIN, GEORGE G., Engineer-Captain, R.N., Deputy Engineer-in-Chief, Admiralty, *Whitehall.*
†GÜMBEL, L. G., Vice-Director of the Norddeutsche Maschinen und Armaturen Fabrik, *Bremen, Germany.*
HAM, JOHN W., Eng.-Comm., R.N., Engineer Assistant to Director of Dockyards, Admiralty, *Whitehall.*
HASTIE, PETER, Chief Draughtsman to Messrs. J. S. White & Co., *Cowes, Isle of Wight.*
*HILLS, H. B., Assistant to Chief Superintendent, New York Shipbuilding Co., *Camden, U.S.A.*
HYNE, A. E., Engineer-Lieutenant, R.N., Admiralty, *Whitehall.*
ISHERWOOD, J. W., Director of Messrs. R. Craggs & Son, *Middlesborough.*
JONES, T. T., Assistant Superintendent, Tranmere Bay Development Co., *Birkenhead.*
KNIGHT, C. W., Assistant Constructor, Admiralty, *Whitehall.*
*KWONG, WONG, Marine Superintendent to the Yangtze Engineering Works, *Hankow, China.*
LUMLEY, GASCOIGNE, Deputy Superintendent Engineer, Government Dockyard, *Southern Nigeria.*
*MACPHERSON, JOHN, Naval Architect to the New Engineering and Shipbuilding Works, *Shanghai, China.*
MCLAURIN, JOHN, Engineer-Commander, R.N., Admiralty, *Whitehall.*
MCMILLAN, W. M., Director of Messrs. Archibald McMillan & Son, Ltd., *Dumbarton.*
MITCHELL, JOHN A., Manager to Messrs. Scott & Co., *Greenock.*
MIZUTANI, Y., Chief Engineer, I.J.N., Kure Naval Dockyard, *Japan.*
NAKASHIMA, Y., Engineer-Captain, I.J.N., Yokosuka Naval Dockyard, *Japan.*
PATERSON, JOHN, Naval Architect, Messrs. John Brown & Co., Ltd., *Clydebank.*
RAWLINGSON, W. W. H., Engineer-Commander, R.N., Chief Engineer, H.M. Dockyard, *Gibraltar.*

* Transferred from Associate-Member Class. † Transferred from Associate Class.

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RECHEA, MIGUEL, Overseer, Spanish Royal Naval Commission, *London.*
RIDER, S., Engineer-Commander, R.N., First Assistant to Engineer Manager, *Devonport Yard.*
RINESI, GIOVANNI, Chief of Drawing Office, Messrs. G. Ansaldo, Armstrong & Co., *Sestri Ponente, Italy.*
ROBERTS, I. E. S., Engineer-Lieutenant, R.N., Engineer-in-Chief's Department, Admiralty, *Whitehall.*
ROOME, G. W., Engineer-Commander, R.N., Royal Naval College, *Greenwich.*
*SALTER, T. C., Consulting Engineer and Naval Architect, *Liverpool.*
SMART, W. H., Partner in the firm of Messrs. A. C. Hay & Smart, *Liverpool.*
*SOLA, J. C. G. DE, Chief of Naval Construction Work, Compañia Trasatlantica, *Cadiz, Spain.*
STEVENS, EDWIN A., Partner in the firm of Messrs. Cox & Stevens, *New York.*
†THORNYCROFT, J. E., Managing Director of Messrs. John I. Thornycroft & Co., Ltd., *Chiswick.*
*TOBIN, T. C., late of Sir James Laing & Son, *Sunderland.*
WETHERALL, THOMAS, Partner in the firm of Messrs. Mason & Wetherall, *Newcastle-on-Tyne.*
WHAYMAN, W. M., Engineer-Commander, R.N., H.M.S. *Arrogant, Atlantic Fleet.*
WILLIAMS, WALTER K., Engineer-Commander, R.N., Royal Naval College, *Osborne.*

* Transferred from Associate-Member Class. † Transferred from Associate Class.

Associate-Members.

‡BAINBRIDGE, T. L., Draughtsman with Messrs. Swan, Hunter, Wigham Richardson & Co., *Newcastle-on-Tyne.*
BARNWELL, F. S., Outside Assistant to the Fairfield Shipbuilding & Engineering Co., *Govan, Glasgow.*
‡BULKELEY, G., Assistant Constructor, H.M. Dockyard, *Devonport.*
CAWOOD, C. A., Consulting Engineer and Underwriter's Surveyor, *Fleetwood.*
CORNBROOKS, T. M., Naval Architect for the Maryland Steel Co., *Sparrow Point, U.S.A.*
COTTON, A. T., Assistant Superintendent Engineer, *Heysham Harbour, Morecambe.*
COOPER, R. P., with Messrs. Harland & Wolff, *Belfast.*
CRABTREE, WM. F., Managing Director, Messrs. Crabtree & Co., Ltd., *Gt. Yarmouth.*
DOXFORD, WM., with Messrs. William Doxford & Co., *Sunderland.*
GOODYEAR, PERCY, Assistant Constructor, Admiralty, *Whitehall.*
GRAY, GEORGE, Marine Surveyor, *Hull.*
‡HILEY, ALFRED, with Messrs. Vickers, Sons & Maxim, Ltd., *Barrow-in-Furness.*
HORTON, J. C. V., Chief Draughtsman to Messrs. F. J. Trewent & Proctor, *Billiter Buildings, E.C.*
‡LAMBERT, L. C., with Messrs. Yarrow & Co., *Glasgow.*
‡LEASK, JOSEPH, Draughtsman with Messrs. D. & W. Henderson, *Partick, Glasgow.*
MCCALLAN, J. W., Assistant Engineer, in the Royal Indian Marine, *Bombay, India.*
MCDONALD, WILLIAM, Manager to the Singapore Slipway & Engineering Co., *Singapore.*
MCGOVERN, JOHN, Chief Draughtsman to the Tranmere Bay Development Co., *Birkenhead.*
PHILLIPS, G. W., Engineer-Lieutenant, R.N., Controller's Department, *Admiralty.*
REITH, R. B., Naval Architect and Marine Surveyor, *Glasgow.*
ROSENIUS, H. D., Naval Architect with Messrs. Tams, Lemoine & Crane, *New York, U.S.A.*
STANSFELD, L. D., Assistant Constructor, Admiralty, *Whitehall.*
‡STEED, F. H., Assistant Constructor, Admiralty, *Whitehall.*
TURNER, ALFRED, Engineer-Lieutenant, R.N., Controller's Department, *Admiralty.*
‡ZIMMER, A. H. A., Assistant Manager to Messrs. T. H. N. Wichhorst, *Hamburg.*

‡ Transferred from Student Class.

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Associates.

Barth, Daniel N. van Kerval.
Blencowe, Frederick John.
Burlamaqui, Armando, Commander R.N.
‡Corbu, Jack, B.A.
Crompton, Frank.
Dunning, Arthur G.
Gandhi, S. K.
George, Edward J.
Ghirardi, Luigi.
Margesson, W. H. D., Commander R.N.
Morgan-Giles, Frank C.
Nasservanji, H.
Pooley, C. A.
Smith, Henry C.
Trollope, F. G.
Watson, George F.
Williams, Hugh, Lieutenant R.N.R.
Woosnam, Arthur.
Wordingham, Charles Henry.

‡ Transferred from Student Class.

Students.

Adams, Albert.
Artsayooloff, N. C.
Barter, Arthur.
Bentley, Thomas H.
Biles, John H., Jun.
Bottomley, G. H.
Butt, Charles J.
Carter, Charles M.
Cave-Browne-Cave, Thomas R., Eng.-Lieut. R.N.
Child, G. H.
Coskery, David A.
Dight, Sydney R., Eng.-Lieut. R.N.
Evans, H. B. Wyn.
Ewing, Robert.
Froude, William.
Good, N. B. N.
Gray, W. D.
Green, Reginald T.
Hackney, G.
Hansford, R. V.
Harding, W. A.
Hudson, W. H. F.
Hugman, Walter B.
Innes, John.
Kent, James L.
Lethbridge, Lionel.
Lillicrap, Charles S.
McMillan, Stanley J.
Noble, W. E.
Payne, Stephen.
Perring, Harold H., Eng.-Lieut. R.N.
Pether, Robert P.
Sanders, Wilfred G.
Spanner, Edward F.
Sutton, Ernest.
Thompson, J. C.
Westhorp, R. S.
Williamson, Lawrence C.
Wilson, John C. M.

Since the issue of Volume XLIX. the Institution has sustained the following losses by death :—

Past President.

The Most Hon. the Marquess of Linlithgow, P.C., K.T., G.C.V.O., G.C.M.G.

Honorary Vice-President.

John Corry.

Vice-President.

J. McFarlane Gray.

Members of Council.

H. F. Swan, C.B. | Robert Thompson.

Members.

Adamson, P. H.
Copeland, C. J.
Foster, H. M.
Gibb, Andrew.
Haswell, C. H.
James, M.
McFarlane, G.
McGee, D.
McIlwaine, J. H.
O'Halloran, T.

Members (contd.).

Parks, Henry.
Pettigrew, J.
Polson, J. B.
Rait, H. M.
Richardson, J. Wigham.
Rogers, J. S.
Welsh, Jas.

Associate-Member.

Darby, J. T.

Associates.

Anderson, J. G. S.
Bertram, Henry.
Crease, Sir John F., K.C.B., Major-General.
Hill, E. J.
Milburn, Sir J. D., Bart.
Pearce, Sir William, Bart.
Scott, G. J.
Webster, M. P.

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The PRESIDENT (the Right Hon. the EARL of GLASGOW, G.C.M.G., LL.D.): Gentlemen, it is my very great pleasure to announce the name which has been proposed by the Council for your consideration as that of your future President. When I intimated that my resignation was to take place at the end of these meetings, the Council took into consideration who would be the best man to take my place; they felt that the Institution might well invite someone more prominent in public life than they had, perhaps, ever had before. The times move on, and the Institution of Naval Architects requires a man of great power and ability to be able to represent it on all occasions. The name which the Council put forward is that of a nobleman of well-known and tried capacity, who has served his country as one of His Majesty's Ministers, who has had other great responsibilities during his life, and who is recognised as a man of very great ability: I allude to Lord Cawdor. You may have already heard his name mentioned in this connection, and I have much pleasure in informing you that when I wrote to Lord Cawdor and told him that his name had been chosen by the Council to be put before you for election, he sent me a telegram saying: "I gladly accept the proposal of the Council of Naval Architects." You all know, as well as I do, the distinguished place which Lord Cawdor holds in Parliament, and the high posts he has filled in the country, and I think it needs no further words from me to put his nomination before you. I can only say, for myself, that I feel it a great honour to be succeeded by such a man, if you elect him, and I think that if you do so you will be fortunate in having so distinguished a President. I now beg to move that this Institution elect the Earl of Cawdor as its future President.

Dr. F. ELGAR: Gentlemen, I have much pleasure in seconding that.

(The Resolution was put to the meeting and carried unanimously.)

The PRESIDENT next put to the meeting the list containing the names of the President, Vice-Presidents, and Treasurer for the ensuing year, which was unanimously adopted:—

President.

THE RIGHT HON. EARL CAWDOR.

Past Presidents.

THE RIGHT HON. LORD BRASSEY, G.C.B., D.C.L.
THE RIGHT HON. THE EARL OF GLASGOW, G.C.M.G., LL.D.

Honorary Vice-Presidents.

H.R.H. GEORGE, PRINCE OF WALES, K.G., K.T., K.P., G.C.V.O., G.C.M.G., Admiral.
THE RIGHT HON. EARL SPENCER, K.G.
THE RIGHT HON. THE EARL OF SELBORNE, G.C.M.G.
THE RIGHT HON. LORD GEORGE HAMILTON, G.C.S.I.
THE RIGHT HON. LORD JOHN HAY, G.C.B., Admiral of the Fleet.
THE RIGHT HON. SIR JOHN DALRYMPLE-HAY, BART., G.C.B., D.C.L., LL.D., F.R.S., Admiral.
*SIR NATHANIEL BARNABY, K.C.B.
SIR JOHN GLOVER.
SIR ANDREW NOBLE, BART., K.C.B., F.R.S., LL.D., D.Sc.
*SIR W. H. WHITE, K.C.B., LL.D., D.Sc., F.R.S.
F. K. BARNES, ESQ.
PROFESSOR J. H. COTTERILL, M.A., F.R.S.
JAMES DIXON, ESQ.
*F. ELGAR, ESQ., LL.D., F.R.S.
R. E. FROUDE, ESQ., LL.D., F.R.S.
HENRY MORGAN, ESQ.

* Members of the House Committee.

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Vice-Presidents.

*The Right Hon. Lord Pirrie, P.C., LL.D., D.Sc.	*Archibald Denny, Esq.
*Engineer Vice-Admiral Sir John Durston, K.C.B.	David J. Dunlop, Esq.
*Sir J. I. Thornycroft, F.R.S., LL.D.	*James Dunn, Esq.
*Sir Philip Watts, K.C.B., F.R.S., LL.D., D.Sc.	R. H. Humphrys, Esq.
*Professor J. H. Biles, LL.D.	*John Inglis, Esq., LL.D.
*H. J. Cornish, Esq.	*J. T. Milton, Esq.
	*W. E. Smith, Esq., C.B.
	*A. F. Yarrow, Esq.

* Members of the House Committee.

Treasurer.

F. Elgar, Esq., LL.D., F.R.S.

The Secretary then read the Scrutineers' report on the result of the ballot for the election of Vice-Presidents, Members of Council, and Associate Members of Council :—

To the Right Hon. the Earl of Glasgow, G.C.M.G., LL.D., *President.*

April 2, 1908.

My Lord,—We have counted the votes recorded on the balloting sheets enclosed in the ballot-box given to us this day, and beg to report that the result of the counting is that the following gentlemen have been elected :—

Vice-Presidents.

Doxford, Sir Theodore.	Oram, H. J., Engineer Vice-Admiral, C.B.

Members of Council.

Barnaby, Sydney.	Fife, William.	Laing, Andrew.
Bell, Thomas.	Gracie, A.	Morison, D. B.
Butler, R. J., C.B.	Gravell, John.	Ward, John.
Deadman, H. E., C.B.	Kennedy, Sir A., F.R.S., LL.D.	Williamson, Sir James, C.B.
Dunlop, J. G.	King, J. Foster.	

Associate Members of Council.

Graham, Marquess of, C.V.O.	Inverclyde, Lord.	Ismay, J. Bruce.

We are, my Lord,

Your obedient servants,

H. R. CHAMPNESS,
H. E. DEADMAN, } *Members of Council.*

C. M. B. DYER,
J. FORTESCUE FLANNERY,
WILLIAM GRAY, } *Members.*

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PRESIDENT'S ADDRESS, 1908.

GENTLEMEN, in the Annual Report of the Council you have heard a very satisfactory account of our work during the past year, and I am happy to be able at the opening of this our Forty-ninth Session, to congratulate the Members of the Institution upon its flourishing condition from every point of view. The accession of new members continues to strengthen our ranks, especially on the professional side, notwithstanding the more difficult conditions of entry, and I am pleased to announce that the list of new candidates that have just been elected is the largest yet dealt with at any Spring Meeting of the Institution. This is the more satisfactory as the last two decades have seen the birth and growth of a number of societies, in France, Germany, the United States, and elsewhere on identical lines with our own, while the allied Institutions at home continue to flourish and supply the needs of local centres where interchange of ideas, and discussions on topics of professional interest, can be carried on during the major portion of the year. Our Institution is peculiar in that so large a proportion of its members being resident either in the outports or abroad, it would be difficult to bring them together otherwise than is now done, at the annual gatherings in London, and at the occasional Summer Meetings which have formed a distinctive feature of our sessions for many years past.

Our Meeting at Bordeaux last year has already been alluded to in the annual report, but I should like to emphasise our appreciation of the cordiality of our reception there and at Le Creusot, a feeling which was generally expressed on all sides by those who attended the meetings and the subsequent visits. The forthcoming Franco-British Exhibition in London will, I hope, further cement the bonds of friendship already existing between the two nations, and give some of our great engineering firms an opportunity of reciprocating the welcome and showing our friends from across the Channel and elsewhere the products of their skill and enterprise.

That the past year has not been one of continuous expansion in the shipbuilding and marine engineering trades is now a matter of history. Indeed, a reaction from the exceptional activity of the previous few years had been foreseen and predicted by those best acquainted with the prevailing conditions. To those who follow year by year the progress of these industries it must be obvious that the laws of supply and demand are as inexorable as ever in governing the fluctuations of trade. Years of high freights and low prices of materials create an abundance of shipbuilding orders which go to swell the output of the following year or two, but an inevitable reaction follows when the demand for freight carriers slacks off and the completion of orders on hand leaves building slips empty and shops running half time. But in addition to the causes operating in, so to speak, a normal manner, there came last year very suddenly, though not perhaps entirely unexpectedly, the financial panic in the United States, which entailed an awkward period of stringent money and high rates, and caused much uneasiness throughout the financial circles of the world. This naturally had its immediate effect upon trade generally, and the shipbuilding and engineering interests suffered with the rest. Then, labour troubles inevitably follow closely in the wake of contraction of business. The workmen, willing enough to take advantage of the flood-tide of prosperity, find it difficult to realise that the ebb has also to be reckoned with, and often refuse to accept the inevitable reduction in wages that must take place if work is to be

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carried on during slack times. It was recently stated that, apart from obsolete and new berths, over 50 per cent. of the shipbuilding berths on the North-East Coast were vacant, and the outlook at that time was extremely unpromising. Since then the strike has assumed very formidable proportions, and has caused much distress in the neighbourhood through loss of orders and consequent employment.

In the aggregate, the falling off in tonnage launched during the past year represents 10 per cent. of decrease upon the totals of the previous year, which was the highest on record for merchant ships. The decrease in tonnage under construction at the close of the year was, however, much greater, there being nearly 20 per cent. less work on hand than that at the close of 1906. One feature of the year's shipbuilding has been the proportion of vessels built for abroad, which were nearly 50 per cent. above the normal supply of recent years. This demand has now been mostly met, and the outlook from this point of view is not much better than from that of home consumption.

But if the year's records have proved disappointing as regards quantity, the same cannot be said of the nature of the work turned out, more especially if we include in our comparison ships whose trials took place during the year under review. We have during that period witnessed some remarkable developments in maritime power, in a mercantile as well as a Naval sense. The rapidity with which improvements in the design of ships or the means of propulsion have succeeded each other during the past decade has culminated in the achievements of the past twelve months. The notable success of the great Cunarders now running with regularity on their Ocean course has reflected the greatest credit upon the constructive powers of our builders of high-speed Ocean liners. No hitch occurred to mar their continuous progress from the building slip to the quay side, and their performance since the start bears splendid testimony to the care with which the details were worked out and obstacles were foreseen and surmounted. The fulness of time has once again brought the dreams of early pioneers into the region of accomplished fact, and the brilliant but costly experiments of Brunel and Scott-Russell have been followed by the great high-speed vessels that have reduced the Atlantic passage to the present minimum of time and discomfort. Times have changed since Dickens' first memorable voyage across the Atlantic, and still more since Dr. Johnson described a ship as "worse than a gaol." "There is," he says, "in a gaol, better air, better company, better convenience of every kind, and a ship has the additional disadvantage of being in danger!" What would this splendid grumbler have thought of the *Lusitania* and *Mauretania?*

The abnormal displacement of these vessels, which swelled the returns of tonnage launched during 1906, makes the biggest ships of the past year appear small in comparison, yet the number of vessels under construction of over 10,000 tons is still relatively high, and the average size is, generally speaking, on the increase.

Special types of vessels for particular classes of trade continue to claim the attention of naval architects, and boldness and ingenuity have characterised the designs of recent freight carriers. Each year sees fresh advances made towards economy in weight of material, the freeing of hold-spaces from pillars or other obstructions, the increase in the working length of hatchways, and the rendering of vessels more sea-worthy when in light condition by the better disposal of water-ballast. The North-East Coast has always been prominent in giving attention to these matters, and the very large and increasing output of that type of cargo-carrier which has become a speciality of the district is significant of the success it has met with among shipowners. A new form of longitudinal construction, which has been recently designed, and for which special advantages are claimed, forms the subject of one of our papers at these meetings.

In marine engineering the past year has been noteworthy for the continued and increased employment of the steam turbine for various classes of ships, both naval and mercantile, and attention is still focussed upon this form of engine. Its success in the Navy, where it has now entirely superseded the reciprocating engine, has been very marked of late. The First Lord of the Admiralty, in his

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explanatory statement, says that "all war vessels at present under construction are being fitted with machinery of this type, which has, without exception, proved very successful in the vessels which have been so fitted." The adoption of the turbine has rendered possible that increase of speed which in every type of warship appears as one of the most conspicuous features of recent construction. The 21 knots of the *Dreadnought* has become the general standard of battleship speed in other navies as well as our own; the modern cruisers, notwithstanding their heavy armour and armament, are enabled to do their 25 knots an hour, while in the torpedo-boat and destroyer classes some very remarkable results have been obtained with turbine engines and oil fuel during the past few months. The so-called "Tribal" class of destroyers has achieved marked success in this respect. Designed for a speed of 33 knots, they have in most cases exceeded this, one of them—the *Tartar*—having maintained, on a six hours' continuous run, an average speed of 35·36 knots. In the case of these destroyers, it must be remembered that the conditions of trial were exceptionally severe, and that the fuel consumption came out satisfactory at both full and cruising speeds.

A still speedier though much larger type of destroyer—the *Swift*, of 1,800 tons, and with a designed speed of 36 knots—was recently launched at Birkenhead, and will shortly be undergoing her trials. It is confidently anticipated that she will, when completed, be the fastest vessel afloat. What is even more important, however, than mere trial speed is the maintenance of speed on service, and from the results obtained it is hoped that these recent types of torpedo vessels will be more successful in this respect than some of their earlier prototypes.

The exclusive use of oil fuel, which has become general in the boilers of these vessels, is an interesting feature of their design, and the engineering department at the Admiralty are to be congratulated on having successfully overcome the difficulties connected with the use of this fuel—difficulties which at one time appeared almost insuperable. How far reliance upon oil fuel may prove to be rightly placed, in view of the limited sources of supply, and their general location outside His Majesty's dominions, is a point upon which there appears to be some divergence of views, but the matter may, I think, be safely left to our technical advisers.

Before leaving the subject of marine engineering, I would call attention to the revival of interest in the superheating of steam for use in marine engines. Constant research along lines likely to lead to economy in marine propulsion has caused engineers to give attention once more to this problem, which was first taken up some fifty years ago, but without reaching a satisfactory solution. New conditions, however, have since arisen, and superheaters have recently been designed for use on board ship which appear to answer the requirements on service, and conduce to considerable economy of steam, and consequently of fuel. A paper on this subject is to be read by one of our distinguished foreign members, which, I am sure, will be received with interest, and will, I hope, throw further light upon the matter.

A few remarks upon the Navy Estimates that have recently been laid before Parliament may not be out of place. The Navy Estimates this year have been awaited with more than usual interest. The sea of controversy that has raged of late about the rock of British Naval Supremacy has created a feeling of uncertainty with regard to the measures that were being adopted to ensure the maintenance of our Navy at its relative standard of power and efficiency compared to the other great navies of the world.

The increasing activity in naval construction abroad, and the extension and improvement of ship-building resources outside the United Kingdom, call for the serious attention of those responsible for the naval defences of this country. In the United States there are at the present time twenty-nine warships building, as compared with twenty-two a year ago. These include seven battleships, two armoured cruisers, three scouts, eight submarines, and five destroyers, and represent a total money value

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of over £8,000,000, exclusive of cost of armour and armament. The German programme, which is designed to cover a period of ten years' consecutive building policy, allots an average of over £10,000,000 per annum to new construction and armaments, and provides for four capital ships a year to be laid down for the next few years. In France, a comprehensive programme is being worked to whereby in twelve years' time that navy would possess thirty-eight modern battleships, twenty-six armoured cruisers and scouts, 279 destroyers and torpedo-boats, and 131 submarines. In Russia, the reconstruction of the Navy is being actively pursued.

In the face of facts such as these it must be admitted that the Navy Estimates recently presented to Parliament are remarkable for the moderation they display. The total amount asked for is £32,319,500, being an apparent increase over last year's total of some £900,000; but when allowance is made for the surplus of stores in hand, this figure is reduced to a net increase of only £14,000. The Shipbuilding vote, which, more directly than any other, concerns the members of this Institution, is actually less than it was last year by over £500,000—and £4,100,000 less than the corresponding vote in 1904, since which time it has been steadily decreasing. When it is remembered that the cost of individual units has during this period increased enormously, it is obvious that the number of ships of each class which can be built from this year's programme must consequently be small. Two large armoured ships (one a battleship, the other a cruiser), six fast but small protected cruisers, sixteen torpedo-boat destroyers, and a number of submarines—such is the total amount of new construction proposed. It is quite evident from this programme that the axiom laid down by the Board of Admiralty three years ago with regard to the necessity of building each year four large armoured ships has been abandoned—at any rate for the time being. Moreover, of the sums now asked for the Government only propose spending a very small proportion on new ships during the current financial year.

No adequate reason has, so far as I am aware, been vouchsafed for this change of policy. It has been stated that our present position is unassailable, and that our facilities for rapid construction are a valuable asset. Both those contentions are true for the moment. But our position will only continue to be unassailable if we take the necessary steps for maintaining it; and as regards the value of speed in construction, this must not be over-rated. It is only of value if others do not achieve it. The monopoly—if it can be so called—of rapid building may be ours to-day, but not to-morrow, for no unprotected monopoly can exist for long in the manufacturing world. There is, on the face of it, no intrinsic reason why other nations whose yards are equipped with modern plant and whose demand for skilled workmen is kept fully supplied should not turn out warships in as short a space of time as our own dockyards or private builders. Our relations with foreign Powers—notwithstanding occasional attacks of newspaper fever—are fortunately friendly, and for the moment no serious complications overshadow the political horizon. But no Government can afford to confine to the immediate necessities of the moment the demands which it must make upon the resources of the nation to provide for the needs of the future. And yet this is apparently what is being done at the present time. "Sufficient unto the day is the evil thereof" appears to be the principle upon which this year's Naval Estimates have been based. I venture to say that no more mischievous principle could be applied to our national defences. A hand-to-mouth policy is one that can only increase our difficulties in the near future, and a very heavy outlay will be needed before long if our standard of naval strength is to be maintained. That standard is admittedly on a rapidly falling gradient as regards new construction at the present moment.

No one doubts for a moment that, should urgent necessity arise, the Government of the day, to whatever party it might belong, would readily vote such sums as its official advisers considered needful. The country would not be satisfied with anything less. But to found a policy of procrastination upon the uncertain value of this eleventh hour remedy is surely to jeopardise our naval position, and saddle the future with an unknown and possibly unlimited liability.

I have been led to speak strongly upon this point, as I feel convinced that the only real economy possible in naval affairs is that which is consistent with the maintenance of our naval superiority.

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With the resources at their disposal, it must be admitted that the present Board of Admiralty have displayed great energy in carrying through various measures for maintaining our naval construction in the forefront of the navies of the world, and the wisdom of a forward policy in the constructive and engineering departments at Whitehall has been fully vindicated. The successful introduction of far-reaching changes in mechanical forms of propulsion is indicative of the high standard of technical knowledge and practical experience attained at our great naval centres. In our private ship and engine building establishments and armour-plate works that standard is nowise lower; the mechanical plant in both Government yards and private works has of late years been brought very generally up to date; our supplies of skilled workmen are still large enough to meet all probable demands, and their ranks are being continually re-enforced through the increased facilities now available for technical education. Such factors as these are national assets upon which we may base the estimate of our resources in the domain of naval construction. Provided only that the necessary funds are voted by Parliament, we need have no fear for the quality of the ships and material that contribute so largely to the maintenance of our supremacy afloat.

Before proceeding to the business of the meeting, I wish to take this opportunity of alluding to a matter of more special interest to myself. It has now been my privilege to address you from this Chair for upwards of seven years, and, much as I have valued the honour of presiding over the interesting meetings it has been my good fortune to attend, I feel that the time has come when a change in the occupancy of the Presidential chair might well be made. The calls upon my time seem to increase rather than diminish as the years roll by, and make it difficult for me, living at a distance from London, to attend the meetings of the Council as frequently as I should like to do. Moreover, there is no doubt that the vitality of an Institution such as ours is best kept up by occasional infusion of "new blood" at the head of its affairs. It is with feelings of real regret that I came to this conclusion, which I have, as stated in the Annual Report, already communicated to the Council. I find it hard to put into words my feelings on quitting the office which I have esteemed it my highest privilege to have held for the last seven years, but I can truly say that I look back upon my period of office with unmixed pleasure. I have had from my colleagues on the Council the advantage of their unvarying support. kindness and consideration, and from the Secretary and his staff able and willing assistance at all times. I can bear testimony to the harmony and goodwill that have characterised our meetings, and I can recall, I am happy to say, no untoward incident during the period of my connection with this Institution.

I am well aware of my own limitations, but as far as lay in my power, I have endeavoured to further the objects for which this Institution was founded. I may, perhaps, be permitted, in taking leave of the office of President, to observe with satisfaction that the Institution, judged by its well-attended meetings, by the valuable papers and discussions recorded in the Transactions, and its ever-growing membership, continues to fulfil its useful function and to attract members from all those parts of the world where shipbuilding and marine engineering claim attention. That it will still continue to do so under the able guidance of its new President and of a Council so fully representative of the industries with which it is associated, I have no doubt whatever. My interest in its future will remain unabated, and it is with feelings of entire confidence in their realisation that I will conclude these remarks by offering my heartiest good wishes for the success and prosperity of the Institution of Naval Architects.

PRESENTATION OF PREMIUMS.

The PRESIDENT then presented the Premiums of the Institution for the past year to Mr. JAMES HAMILTON GIBSON for his paper on "Torque of Propeller Shafting: Some Investigations and Results," and Mr. LYONEL CLARK for his paper on "Modern Floating Docks."

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At the conclusion of the Annual Dinner, on April 8, 1908, an illuminated address was presented, on behalf of the Council and Members of the Institution to the retiring President, by Dr. F. Elgar (Honorary Vice-President). The following is the text of the address :—

ADDRESS TO THE RIGHT HON. THE EARL OF GLASGOW, G.C.M.G., LL.D.,

President of the Institution of Naval Architects, 1901–1908.

We, the undersigned, on behalf of the Council and Members of the Institution of Naval Architects, desire to express to your Lordship, upon the occasion of your retiring from the office of President, our high appreciation of the great services you have rendered to the Institution during your period of office.

The present flourishing condition of the Institution, and the continued expansion of its representative character in the field of Naval Architecture and Marine Engineering are the best possible proofs of the able guidance of its retiring President. During the seven years that your Lordship has held this office, the Council and Members have respected and valued your never-failing courtesy, tact, and kindness, and the impartial attitude which you have consistently maintained in the performance of your important duties.

We hope that for many years to come you may enjoy both health and happiness, and we trust that you will continue, as one of our distinguished Past Presidents, to take an interest in the successful future of the Institution.

(Signed)

JOHN DALRMYPLE-HAY,
NATHANIEL BARNABY,
JOHN DURSTON, } *Vice-Presidents.*

FRANCIS ELGAR, *Treasurer.*

R. W. DANA, *Secretary.*

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UNSINKABLE AND UNCAPSIZABLE SHIPS OF THE GOULAEFF FORM AND SYSTEM OF CONSTRUCTION.

By General E. E. GOULAEFF, F.R.S.N.A., Member.

[Read at the Spring Meetings of the Forty-Ninth Session of the Institution of Naval Architects, April 8, 1908; the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D., President, in the Chair.]

IT is only since the terrible loss of H.M.S. *Victoria* by capsizing (a disaster repeated in several cases during the late Russo-Japanese naval war) that it has at last been recognised that, as soon as a ship of the present ordinary form, proportions, and system of construction receives a blow either by ram, torpedo, shot, or collision, perforating the underwater part of the hull and causing more or less serious injuries, the vessel immediately heels dangerously over and loses her stability. Indeed, owing to the fact that such underwater openings seldom occur exactly in the middle of her length, but mostly at some distance forward or aft, the vessel nearly always rapidly acquires, at the same time, a considerable longitudinal trim, and gets depressed either by the bow or stern, immersing part of her upper structures; that at once jeopardises her longitudinal stability also, since the useful portion of the area of the load water-line is seriously and very quickly diminished by the inrush of water. By these means the vessel gradually and very rapidly loses whatever stability she might have had originally, and finally upsets or capsizes so rapidly that there are cases recorded in which the actual time of capsizing amounted to less than two minutes. The helpless condition to which many modern warships of various nationalities might be reduced has been actually proved by calculation and model experiments both in Russia and America; while, in the recent war with Japan, some ironclads of the *Borodino* class were seen floating after the battle of Tsushima bottom upwards, looking like the backs of some huge species of fish!

Ever since the loss of the *Victoria*—that is, for more than ten years—I have thought how to solve this problem in the most practical manner, and it is the results of my work in this direction that I wish to bring before the Institution of Naval Architects, trusting that the discussion, after the reading of this paper, may prove that the adoption of the proposed form and system of construction will give us *practically unsinkable and uncapsizable ships, even in the presence of numerous underwater openings.*

Until the present time the most powerful weapon against which we have to specially protect our ships is the torpedo, fired either from torpedo craft or, still worse,

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from the invisible submarine boats. Moreover, these torpedoes are being continually improved in speed, dirigibility, and weight of bursting charges, and these latter are also being rapidly improved as regards their destructive effect per unit of weight. The external net-defences, so much thought of formerly, cannot, since the *Belleisle* trials, be considered to effectively answer their purpose any longer, as they were partly burnt and melted by shell fire during these trials. They possess, besides, many disadvantages, as they are cumbersome, difficult, and slow to put in place, and they lessen the speed of the vessel when moving. Being entirely in sight of the enemy, they are liable to be soon destroyed by the fire of comparatively light guns. In the proposed system of construction, comprising, as will be seen from the description, some modification of the form and proportions of vessels, I have endeavoured, on the contrary, to protect the ship against this weapon by the internal system of construction of the hull.

I have attained this purpose by making vessels much broader than they have been or are at present, leaving their length the same, or making them even somewhat longer. I shall not trouble the Institution with a detailed description of this form and system of construction, which provides treble broad longitudinal cellular side corridors, which are rendered possible by the increased breadth of the vessel, and are intended to reduce to a minimum the quantity of water that may enter the ship through injuries or openings made in her underwater skin. Fig. 1 (Plate I.) gives a general idea of the transverse sections and plans of hold of a ship of this system of construction. The increase of breadth is made at the expense of the draught of water, so much so that the ratio $\frac{B}{D}$ amounts to about 4·5, instead of 2 to 3, as in vessels of ordinary form and proportions. The displacement is practically the same as that of a ship of ordinary form, with which the comparison is made. In the wide treble side there are safety longitudinal corridors, shown in Fig. 1, surrounding nearly the whole length of the vessel, and I propose to have, as far as practicable, *no watertight or other kind of doors*, but only the smallest possible, permanently closed boiler manholes, for the purpose of giving access to the cellular compartments from the top. As members of this Institution will remember, the introduction of the double bottom and double sides, in the early sixties, was due to the remarkable genius of the late Sir Edward Reed, and this innovation was undoubtedly of the greatest importance at that time, when the torpedo had scarcely come into general use. Nowadays, when the conditions of naval warfare have changed so much, and when the injuries that can be produced by a single torpedo are so terrible, some more efficient means must be found to protect ships from this most powerful weapon. The radius of action of the modern torpedo, inside a vessel, counting from the outer skin inwards, being about 18 ft., I propose to make the width of the cellular side corridors about 18 ft., that is 6 ft. for each corridor. Thanks to this, all the internal vital parts of the ship, especially those liable to explosion, such as steam boilers, magazines, shot and shell rooms, and torpedo store-rooms, &c., are removed from the outer skin for a distance of about 18 ft. inwards on each side towards the centre of the vessel, thus very efficiently securing their greater

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safety from any outside explosion, or any other mode of attack. In larger ships I would suggest having this distance increased to about 20 ft.

(I.) *Resistance of Water to the Motion of Vessels of Great Beam.*—When, in 1896-1897, I brought forward my proposal for such ships, I was met by the criticism that the resistance of water to the progress of such broad vessels at any high speed would be altogether too great. But, knowing the results of the late Mr. W. Froude's investigations of broad and shallow forms, I felt sure that I was right, at least in this respect.

(*a*) The diagram in Fig. 2 (Plate I.) shows a curve R R of the resistance, or of effective horse-power at different speeds from 13 to 20 knots, of the Russian ironclad *Retvizan*, of ordinary form and proportions, compared with a similar curve G G for an ironclad of my own form, and of the same displacement. This diagram, as well as those described below, are based on the results of model experiments made in the St. Petersburg Experimental Tank, and the curves given in the resistance diagrams are the results of the trials of the models of several ships of the forms stated on the diagrams. Fig. 2 shows that, whilst the displacement of both vessels is the same (14,266 tons), the indicated horse-power of engines required to drive a ship of the *Retvizan* shape at a speed of 18·4 knots amounts to 23,600, whilst the indicated horse-power required to drive a ship of the broader and shallower form, at practically the same speed of 18·42 knots, amounts only to 19,412, and this difference in favour of the broader vessel increases if the speed be further increased.

(*b*) Fig. 3 shows a comparison of a later and better-shaped Russian ironclad, the *Borodino*, still of an ordinary form, with an ironclad of my form and proportions, both brought, as in the first case, to the same displacement of 17,220 tons. In this case, we notice that the point of intersection of both curves occurs at an ordinate corresponding to a little higher speed, viz., 18·5 knots (instead of 17·5 knots as in Fig. 2). In this case the indicated horse-power required to drive the ship of ordinary (*Borodino*) shape at a speed of 19·65 knots amounts to 30,400, whilst the indicated horse-power required to drive the broader and shallower ship, even at a little higher speed of 20 knots, amounts only to 26,920.

(*c*) Fig. 4 shows the comparison of the curves of effective horse-power for a very fast protected Russian cruiser, *Askold*, of ordinary form, and the curve G G G for an armoured cruiser of the same displacement (13,180 tons), of my own form, specially designed for high speeds. In this case the superiority of the proposed ship is still very striking, as shown by the fact that the point of intersection of these last two curves occurs at so low a speed as about 10 knots. Beyond this limiting speed, and up to 25 knots and over, the curve G G G keeps below the curve A A A, which latter, as in the case of the two previous diagrams referred to, has an *almost asymptotic character the higher the speed grows.*

(*d*) Fig. 5 shows the curves C and D of the effective horse-power of a large ocean steamer of my form and proportions at her two different draughts of water, viz.,

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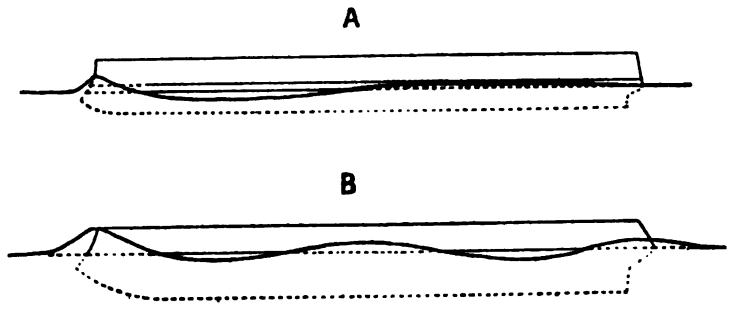
18·33 ft. and 20·33 ft., of a normal displacement of 23,000 tons (S.S. *Deutschland's* displacement), amongst a group of similar curves of effective horse-power belonging to different steamers of ordinary form, viz.:—Curve A, *Grozyaschy;* B, *Svietlana;* E, *Good Hope;* F, *Merkara;* and G, *Kaiser Wilhelm der Grosse;* all brought to the same displacement of 23,000 tons. It is interesting to note, *en passant*, that the small difference between the ordinates of the curves C and D shows that even considerable overloading (about 2,700 tons) of a vessel of my form will not affect her high speed very materially, which is no small advantage for a merchant steamer, especially as the displacement per inch immersion in the vessel of this form and proportions is almost double that of a vessel of ordinary narrow form. In this case the difference of indicated horse-power required to drive these vessels at the same speed of 25 knots amounts to from 20,000 to 24,000 I.H.P. in favour of a vessel of the broader and shallower form, which is very important, as showing again that, *the higher the speed at which we make our comparison, the more advantageous becomes my form of ship, in respect of the resistance of water.* This favourable result is no doubt obtained owing to the considerable reduction in the draught of water, and, consequently, of the pressure of water against which the ship has to labour in her progress, the vessel, so to speak, *skimming over* the water's surface.* The superiority is so great that, as shown by this diagram, a steamer of my form would be capable of developing nearly 27 knots as compared to 25 knots for a steamer of ordinary form, of the same displacement, and with the same power of engines.†



The sketch on this page shows the difference of the wave formation accompanying the motion of a vessel of my form A, and that of a ship of ordinary form B, when both are driven at the same speed of about 20 knots. The wave formation in the first case is remarkably less than in the second, and this absence of wave-making clearly

* This same reason, no doubt, accounts also for the excellent results of Mr. Yarrow's motor torpedo boat having a ratio of $\frac{B}{D} = 9$ (that is even greater than in the ships of my proposed form and proportions), and also a flat bottom.

† All the curves of effective horse-power in Fig. 5 (Plate I.) were drawn by the staff and on the authority of the St. Petersburg Experimental Tank, and were obtained mostly as the results of trials of models of the ships referred to in the diagram, with only three exceptions, viz., of the curve F for the s.s. *Merkara*, the data for which were taken from the corresponding diagrams given by the late Mr. William Froude in the Transactions of the Institution of Naval Architects, of the curve E for the cruiser *Good Hope*, the data for which were drawn from the figures given in the published accounts of this vessel's measured-mile trials, and of the curve G for the German s.s. *Kaiser Wilhelm der Grosse*, the data for which were taken from the Transactions of the German Institution of Naval Architects.

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explains the facts already mentioned in favour of the proposed form of ship, since it is especially with an increase of speed that the wave-making resistance of ships of the ordinary form grows enormously.

The final results of these investigations of ship resistance prove that with the ordinary form of ships we have nearly reached the limit of advantageous increase of speed, beyond which, however much we increase the propelling power of machinery, we get scarcely any adequate increase of speed. Practically this has been proved for several years past by the enormous sacrifices which naval architects have to make in order to obtain the last few additional knots out of a ship of the usual form. The proposed type of ship would, moreover, admit of placing on board, thanks to its great beam, any required number of turbine-driven propeller shafts.

(II.) *Insurance of such Vessels against Sinking and Capsizing.*—Ships of my proposed system of construction possess, as compared with ordinary ones, incomparably greater unsinkability and uncapsizability under the effects of torpedoes, shot, rams, and collisions with other ships. The following table will illustrate this point :—

TABLE I.

—	Displacement, 17,220 tons.	
	Ironclad of the Goulaeff form and system of construction.	H.I.M. Ironclad *Emperor Paul I.* of ordinary form and system of construction.
Angles of heel under the same conditions of underwater injuries made by torpedoes. Extent of injuries, lengthwise 60 ft.	2° 40′	14°
Angles of heel under the same but still more extensive and severe underwater injuries made by torpedoes. Extent of injuries, lengthwise 170 ft.	6° 50′	30°

From the above table it is evident how much more moderate an angle of heel a ship of my form takes as a result of underwater injuries to the hull, compared with an ordinary ship of the same size, under exactly the same conditions. At the last angle of heel given in the table (30°) of the ironclad of ordinary form, her side turrets enter the water, and the vessel herself becomes immersed to a draught of 33·25 ft. by the bow and 36·12 ft. by stern, with a still further increase in the quantity of water inside of her, pouring in through the above-water ports and through possible openings made by artillery fire in her sides, and spreading along the interior of the 'tween decks, causing her to lose more and more of her stability, until she finally capsizes. Whereas, under the same conditions, as shown in Fig. 6 (Plate II.), an ironclad of my form after *being struck even by several torpedoes* still remains perfectly able to fight her guns or to proceed under her own steam to her destination. The comparative smallness of her angle of heel is shown in Fig. 6,

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representing the results of extensive calculations of the stability of both vessels, with some compartments (those shaded on the drawing) filled with water, the extent of the underwater injuries sustained being taken lengthwise at 60 and 170 ft., with a radius of destruction of the torpedo 18 ft. inwards.

The adoption of this system of construction in mercantile steamers would, therefore, enormously increase their safety in case of collision, and would, consequently, reduce the rates of insurance required by underwriters.

(III.) *Reduced Draught of Water (amounting in big Battleships to some 6 or 7 ft., and an even greater reduction in large Merchant Steamers).*—In ships of ordinary form the limit of draught available has already been practically attained by the limiting depth of water in certain important channels and ports, unless we resort to a vast amount of artificial dredging. This would take time, and would, moreover, be too costly. Rather let us adapt our ships to that depth of water which is given us gratis by nature. My form and system of construction would not only satisfy this condition, but would greatly reduce the number of cases of ships grounding on shallows or rocks, thus offering additional security for navigation in shallow seas and intricate channels.

(IV.) *Increased Internal Capacity.* — Increased breadth insures so much additional internal capacity that great quantities of fuel or other cargo may be stowed on board such ships as compared with ships of ordinary narrow form, and this favours the adoption of such ships either for distant cruising purposes or for carrying a large cargo in hold, and passengers above in the central upper structure. In a tentative design of an ocean steamer of high speed, of my form, thanks to the height to which her central superstructure could be raised, most of the passengers—numerous though they were—could have been each accommodated with an excellent and spacious state-room, and the natural lighting and ventilation much improved besides.

(V.) *Safety against Explosion of the Ship's own Magazines and Torpedo Store-room.*—The safety cellular corridors so effectively isolate all the vital parts and stores of the ship liable to explosion and ignition, such as magazines, torpedo store-room, &c., from the outer skin of the vessel, that the blowing up of these by *detonation* from the explosion of an enemy's torpedo or mine outside in the vicinity of these parts is most efficiently prevented—just such an explosion as caused the foundering and capsizing in the late war of H.I.M.S. *Petropavlovsk*, with her gallant Admiral Makaroff, and nearly all his officers and crew.

So far, I have only described and discussed *the positive or superior qualities of the vessels of the proposed form and system of construction*, restricting myself to their main features and properties. I will now discuss those few features which have raised doubts in some quarters. These are :—

(VI.) *Sea-going Qualities of these Ships and their Steadiness of Platform.*—It has been urged that, owing to the increased breadth of such ships, their stability,

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measured by the metacentric height, being increased, they should be less steady in a sea-way than narrower and less stable ships of the ordinary deep form. This, of course, has been taught in the elementary text-books, in which our generation studied Naval Architecture, but there are new theories which have at last thrown more light upon this complex question of the rolling of ships upon waves of particular size and character. Having studied this question in connection with the rolling of ships of my form, I will now submit the following interesting facts and data :—

(*a*) Mr. Ivan G. Boobnoff, of the I.R.N., a member of this Institution, has described the probable rolling of the vessels of my form in the following words :— "These broad ships, possessing as they do a very large co-efficient of extinction, will have the transverse rolling more similar to the longitudinal rolling of ordinarily shaped ships, that is, a uniform one, without stoppages, and of the same period as the period of the waves. These ships' own oscillations will be reduced by their great breadth, as well as by other peculiarities of their form, so that the ship will roll uniformly, though rather briskly, but with small amplitudes."

Basing his calculations on Mr. R. E. Froude's formula, given in his valuable paper on "Non-uniform Rolling," Mr. Boobnoff has calculated the figures given in the following Table II. of comparison of the amplitudes and angular velocities of the rolling of two typical ships, viz., of H.M.S. *Revenge* of the ordinary form, and of an ironclad of the broad and shallow form.

TABLE II.

Period of the waves in seconds ..	4	6	8	10	12	14	16	18
Length of the waves in feet.. ..	82	185	328	513	738	1000	1300	1660
Amplitudes :—								
Maximum for H.M.S. *Revenge* ..	1° 3′	3° 2′	6° 0′	10° 8′	16° 3′	22° 5′	28° 0′	23° 0′
Maximum for Goulaeff's ship ..	2° 1′	6° 0′	9° 0′	10° 0′	10° 0′	10° 0′	10° 0′	10° 0′
Angular velocities of oscillation, degrees per second :—								
Maximum for H.M.S. *Revenge* ..	0° 5′	1° 3′	2° 4′	4° 3′	6° 5′	9° 0′	11° 2′	9° 3′
Maximum for Goulaeff's ship ..	3° 0′	6° 0′	7° 1′	6° 3′	5° 2′	4° 5′	4° 0′	3° 5′

N.B.—In this Table the maximum wave-slope was taken by Mr. Boobnoff in all cases as equal to 10°, and the mean period of the waves in closed seas as not exceeding 10 seconds.

The general character of the rolling of both vessels compared is shown in Mr. Boobnoff's diagram on the next page, from which we can judge *of the superiority of the rolling of a vessel of my form and type in comparison with the rolling of a ship of ordinary form, both in respect of the regularity or uniformity of rolling motion, and of the comparative smallness of the amplitudes of oscillation.* Indeed,

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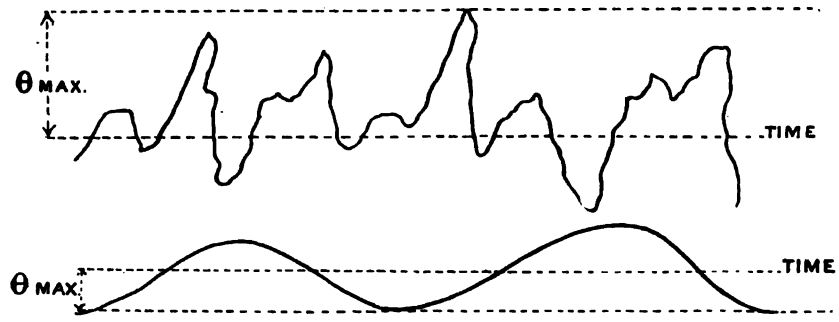
judging by this diagram and Table II., and seeing that the oscillations of my ship are so remarkably regular and uniform and become less and easier the bigger grow the waves (second half of the Table II.), we may expect that *the accuracy of pointing and firing guns mounted on board such vessels will be very much better attained than that of the guns mounted on board a vessel of ordinary description*, and this is a very great advantage for ships of war. From Table II. we also see that, although the rolling of the broad ship is not so moderate in amplitude of oscillation, and not so gentle in angular velocity, when rolling amongst small waves of a length from 82 to 513 ft. long and of a period of 4 to 10 seconds, there is undoubted superiority and a very essential reduction of rolling as soon as she finds herself amongst waves of 738 to 1,660 ft. and over, and having a period of 12 to 18 seconds and upwards.



(*b*) Figs. 7 and 8 (Plate II.) were calculated and drawn up by Colonel A. N. Kriloff, I.R.N., Chief of the St. Petersburg Experimental Tank. Whilst the diagram given above represents only the general and approximate or qualitative illustration of the character of the probable rolling of my ships, the diagrams in Figs 7 and 8 show a closer numerical or quantitative comparison of the rolling of the two ships, when both are moving at the same angle (45 deg.) to the direction of the waves, the abscissæ representing the lengths of waves in feet, and the ordinates on the upper figure representing the amplitudes of rolling in degrees, and those on the lower figure, the angular velocities of rolling in degrees per second. Here again we notice the intersection of the curves in the upper and lower figures (indicating a complete change of phenomenon), and that, although for shorter waves the rolling of a ship of ordinary form is more moderate both in respect of amplitude and angular velocity, this superiority stops at the point of intersection of the curves, after reaching which, and, *as the waves increase in length and period, the rolling of the ship of my form becomes less than that of the ordinary ship* both in respect of amplitude and in respect of angular velocity of oscillation.

(*c*) This peculiarity has been most kindly explained to me by Mr. R. E. Froude in a letter on the subject in the following terms:—"I agree with what I take to be your views also, namely, that, starting with a design of ordinary type, with small metacentric height, a moderate increase of metacentric height tends to increase extreme angle of rolling and to increase prevalence of rolling (that is, to widen the range of conditions under which rolling takes place); but that an extreme increase, such as you propose, will tend, perhaps considerably, to diminish the extreme angles of rolling and give a ship freer from severe angles of rolling than one of ordinary type (unless the latter

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is fitted with very large bilge-keels). . . . There is something to be said on both sides, and the question really depends on a balance of evils."

TABLE III.

Length of waves in metres ..	20	40	80	120	160	200	240	280	320	360
Length of waves in feet ..	66	133	263	394	526	657	789	920	1052	1183
Period of waves in seconds ..	3·58	5·06	7·15	8·8	10·12	11·3	12·4	13·4	14·3	15·18
Amplitude of oscillation to port and starboard :—										
Maximum for *Weissenburg* type	—	—	2° 20′	4° 50′	7° 40′	14° 20′	33°	—	43°	25°
Maximum for Goulaeff type ..	0·05°	2° 20′	5° 40′	8°	9°	8° 50′	8° 27′	8° 10′	8° 05′	8°
Angular velocity of rolling :—										
Maximum for *Weissenburg* type	—	—	0·0353	0·0598	0·0834	0·1388	0·2918	—	0·329	0·1732
Maximum for Goulaeff type..	0·0025	0·0512	0·087	0·0995	0·0974	0·0865	0·0734	0·067	0·0621	0·0555

(*d*) Mr. Tekhel, a learned naval architect, has sent me the diagrams shown in Figs. 9 and 10 (Plate II.), with Table III. corresponding thereto, confirming the *reduction of rolling in the vessel of my form and type*, as compared with that of a ship of ordinary form, *as the waves increase in length and period*. He claims special accuracy for his calculations, particularly in relation to the smaller waves. From his diagram, as well as from Table III., it will be seen that, when rolling amongst waves having a length of 1,052 ft. and a period of 14·3 seconds, a ship of my form attains an amplitude of oscillation of only 8°, whilst a ship of ordinary form of the German H.I.M.S. *Weissenburg* class, for instance, rolls to an amplitude of about 43°, the corresponding *angular velocities* of oscillation being in the proportion of 0·062 to 0·329 respectively, that is, about *five times as slow in favour of the ship of my form*. In the preceding column of Table III., relating to waves of 920 ft. and 13·4 seconds period, no amplitude or angular velocity of the *Weissenburg's* rolling is given, because in this case the period of the vessel herself synchronises with the period of the wave, and her rolling is therefore still worse, whilst my ship continues to oscillate with the same uniformity of amplitude (*i.e.*, about 8°) with a moderate angular velocity of 3·84° per second, which latter figure is obtained by multiplying the figure given in Table III. by $\frac{360°}{2\pi}$. Another important feature arising out of this diagram and table is that, whilst the amplitudes of oscillation of an ordinarily shaped ship are subject to such great variation, with the variation in the size of waves, as from 2° to 43°, there is scarcely any variation at all in the amplitudes of oscillation of the ship of my form. As to the somewhat increased velocity when rolling amongst very small waves, my opinion, shared, I believe, by all practical men,

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is that when we come to deal with big ships, especially those moving at high speed, small short waves have much less effect the bigger the ship is, of whatever form she may be.

(*e*) Mr. C. Tennyson, M.I.N.A., studying the same subject, maintains that "a ship of my form, following in her motion that of the waves, will be always remarkably dry, floating like a duck, whatever may be the condition of the waves about her, and therefore she will never be swamped, as this can be prevented by closing all her above-water hatches, side lights, ports, and other openings; persons on board of her will feel as though the ship does not roll at all; and the work on board will not be impeded by rolling."

(*f*) That such will be the actual sea-going qualities of ships of the proposed form, notwithstanding their large metacentric height, is fully confirmed by practical experience as proved by the evidence and opinions of the following high authorities, who expressed themselves on this subject, in this very Institution, at the discussion which followed the reading of the late Sir Edward Reed's paper in the year of 1881 on the behaviour of the yacht *Livadia* in the Bay of Biscay, viz., the late Vice-Admiral Sir Houston Stewart, Sir William White, Vice-Admiral Selwyn, and the late Sir William Pearce.*

In view of the extremely favourable opinions of such authorities in regard to the excellent behaviour of the *Livadia*, only 235 ft. long, 150 ft. beam, drawing only 7 to 8 ft., and therefore possessing, owing to her unprecedented beam, a very great metacentric height, I believe that the application of this form and system of construction to the modern ocean passenger steamers ought to be eminently successful, both as regards high speed on the passage, and also for comfort and safety which passengers would enjoy on board such steamers.

(VII.) In Sir Edward Reed's paper on the *Livadia* in the Bay of Biscay, he pointed out that "a ship of extremely light draught and of almost perfect steadiness receives violent upward blows from the ascending water, and this more especially forward, where the onward motion of the ship naturally subjects the bow to the additional violence. This certainly was the case with the *Livadia* during the gale we then experienced in the Bay." To this I must answer that ships of the proposed form and proportions differ so very much from the *Livadia* in that they certainly draw not less than 20 to 21 ft., instead of, as in the *Livadia*, only 7 to 8 ft., and in being of much larger displacement, greater length, and finer entrance, so that they would behave in this respect more like ordinary steamers in ballast, whose draught of water happens to be at times even less, and which still make very good passages across the North Atlantic, even in the severe winter seasons.†

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* Transactions I.N.A. (1881), Vol. XXII., pp. 271-280.

† See Mr. Thearle's Paper in the Transactions I.N.A. (1903), Vol. XLV., p. 118.

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(VIII.) *Dock Accommodation Difficulty.*—Seeing that ships of the proposed type and of large displacement, such as that of modern battleships and ocean steamers of ordinary form, would certainly be as much and even more than 100 ft. in breadth, objection has been raised as to the difficulty of finding docks wide enough to accommodate such broad ships for the purpose of underwater repair and painting. At first sight this may seem to be a serious disadvantage, but it does not present an insurmountable obstacle, for the necessity for broader docks was keenly felt as long ago as the seventies of the last century, when in the well-known *Inflexible* Committee's Report it was stated :—"We note that the beam of the *Inflexible* was limited by the consideration of the docks available for her repair, but we doubt if this consideration ought to outweigh the great advantages which a further increase of beam would give to vessels of the *Inflexible* type. We are the more inclined to doubt it, because at present docks capable of accommodating vessels of any breadth can be constructed of iron rapidly, and at no serious cost in comparison with that of such vessels as the *Inflexible*."

Indeed, floating docks now offer a most ready and suitable way out of this difficulty, and their cost is so moderate that there was lately an offer from a well known ship-building firm to our Government to build an ironclad of my form, together with a floating dock to accommodate this ship, for exactly the same price as an ironclad of the same size but of ordinary form is being built in a Russian yard. The "Dewey" floating dock, built by the American Government for their new Naval station at Cavite, has an internal width of 100 ft. in the clear, and floating docks of still greater breadth might easily be built.

As for the masonry graving docks, even for modern ships of ordinary narrow form and proportions, we already meet with the difficulty of accommodating ships fitted with bilge-keels of sufficient size to diminish their rolling, on account of the narrow entrances to the existing dry graving docks. Indeed, if we consider the growth in the internal dimensions of even such dry docks, we see that, owing to the constant and uninterrupted increase in the size of ships generally, and particularly in their breadth, for the last 90 years, at intervals of thirty years, viz., 1844, 1874, and 1904, we find an increase of width of docks, such as that shown in Fig. 11 (Plate II.).* The next diagram (Fig. 12) shows similarly the evolution in the dimensions of a floating dock during the period of last century—from 1800 to 1900. The internal width has almost doubled, thus proving the truth of the saying that where there is a demand there will be always a supply. The width of the new No. 5 graving dock at Cherbourg is already 118 ft., that of the new Brooklyn dock 120 ft.

With regard to the width of important navigable channels, it may be added that it is proposed to nearly double the width of the Northern Ship Canal at Kiel, whilst the width of the Suez Canal may be estimated by the fact of its allowing the big American floating dock, having a total breadth of some 125 ft. to be recently towed through it safely.

* From *Engineering*, September 30, 1904.

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DISCUSSION.

Mons. E. BERTIN (Honorary Member): My Lord and Gentlemen, I feel myself peculiarly interested in General Goulaeff's proposed ships, which I hope may soon be built and be entirely successful. I beg to be permitted, therefore, to add a few observations of my own to those which he has given us in his clear and descriptive account. In the first place, General Goulaeff does not seem to have taken into consideration the influence of the open main deck placed at a small height above the water-line, in lessening the amplitude of rolling relative to the normal to the surface of waves, an influence similar to that of very deep bilge keels. Nor does he take into account the effect of the transverse dimensions of the underwater hull and the metacentric height of the ship upon the rolling of the normal to the wave surface itself, by which the angle of the position of equilibrium with the vertical is lessened. Further, he does not take into account, in support of his scheme, the saving which would be made on the weight of armour by reducing the height of the thick protective belt. I will put aside, on account of its complication, the reduction of inclination of the position of equilibrium, which it would not be possible to calculate exactly, and the effect of which would be favourable to General Goulaeff's argument in some cases and unfavourable in others, in proportion to the part played by stability. It is obvious that with ships of small stability there would be a straightening effect by which the position of equilibrium would be upright on the slope of waves, and even inclined in the opposite direction to the slope. The influence of the main deck in reducing rolling at small inclinations and the saving on the weight of armour—these were the chief reasons which induced me to venture upon the building of the *Henri IV*. From both these points of view the advantages foretold were fully realised, and I would take the liberty of drawing General Goulaeff's attention to them. I also beg to point out that the study of stability limited to statical stability is not sufficient. Warships, when damaged in fighting, may retain sufficient statical stability with a small inclination, although they may be in danger of capsizing by the dynamical effect of some moment of inclination, such as the action of the rudder may produce. This has been the case with some ironclads of ordinary form, the unarmoured or lightly armoured superstructure having been partially destroyed. It was fairly proved some thirty years ago in France, apart from any calculations, by the danger of capsizing to which the *Tonnerre*, a ship of large initial stability, but of insufficient height of belt, was exposed when the rudder was suddenly put into action. This danger increases when the area of the water-line decreases from the effect of the enemy's artillery, even without the assistance of the torpedo, the only weapon of which General Goulaeff takes any account in this respect. The protection of stability requisite from this point of view is easily determined, either by calculation or by experiments on models; its necessity has been proved by several catastrophes, such as the sudden capsizing of a Chinese cruiser at the battle of Yalu, a disaster which, in the first accounts of the battle, was attributed to the effects of a torpedo attack. The danger of capsizing from insufficient dynamic stability ought to be a conclusive argument in favour of General Goulaeff's design.

Having made these remarks, which are not intended to be critical, I would urge my approval of this paper, in which a question of the highest importance to Naval Architecture is treated in its fullest sense. In the year 1900 General Goulaeff expressed his approval of the *Henri IV.*, and his astonishment at finding in France, nearly completed, a warship of the very type which he advocated at that time for his own country. I can tell him now, with equal courtesy, how much his present point of view differs from mine. In the year 1895 I decided to begin the design of the *Henri IV.*, owing to the impossibility of obtaining, during the previous five years, assent to the plans for our ironclads of the *Patrie* type, the study of which goes back as far as the year 1890. A displacement of 14,000 tons was then considered unacceptable. Small ironclads with thick armour and big guns, quite capsizable, of course, if of the ordinary form, were the only ones desired. Recalling calculations made during the first months of the year

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1870, in consequence of which I had then proposed a kind of monitor of the required displacement,* with an armour belt of 30 centimetres, and a cellular belt at the water-line, I transformed that very old design by the help of twenty-five years' experience. During this long period I had designed or built nothing but ships of ordinary form, and I had never guessed that the general disposition of monitors could be a convenient one for cruisers or passenger steamers. The model experiments on the resistance of water to the motion of vessels, which have been carried out in the St. Petersburg experimental tank at General Goulaeff's instigation, are of the utmost interest, as they lead, if the results given by the ships confirm those obtained in the tank, to generalise the design first conceived for a class of warships only. The example of the *Henri IV.*, though not quite conclusive, is such as to give hopes to General Goulaeff, for the calculated speed of 17 knots has been easily exceeded, and there is no appearance of an asymptotic speed. With ships of large beam and small draught of water, one would formerly have been checked in attempting to go much beyond 17 knots by the difficulty of giving the screws a sufficiently large diameter. But, nowadays, old rules for the form and dimensions of screws are discarded, and the adoption of turbines for cruisers and passenger steamers creates a situation quite favourable to the proportions of hulls advocated by General Goulaeff.

In conclusion, I beg to make two last remarks, wishing to warn the author of the paper against two possible dangers. For passenger steamers, where the space for the passengers is never considered sufficiently roomy, the profile given in Fig. 1 (Plate I.) would not be easily accepted. For cargo vessels there would be no objection, except the competition of turtle-back ships. For warships, there will always be some objection to this design on account of the amplitude of rolling, however moderate it may be—oscillations of from 2 to 5 degrees for the half period, which are the ordinary conditions met with at sea in fighting weather. Such rolling is troublesome for the gun-layer, and it may place the ship in a state of inferiority against an adversary of ordinary form, which would experience no rolling at all. On big waves and in bad weather, the figures given by General Goulaeff are quite conclusive, and they have been confirmed by the performance of the *Henri IV.* On small waves and in fine weather, the question is not completely settled.

Admiral C. C. P. FitzGerald (Associate Member of Council): My Lord and Gentlemen, unfortunately, I did not hear all that M. Bertin said, and, if I should happen, therefore, to repeat any of his criticisms, I hope you will forgive me. He has, no doubt, approached the subject from a Naval Architect's point of view. The few remarks that I propose to make, I need scarcely tell you, will be from a seaman's point of view. I think the very title of the paper is, to say the least of it, a bold and comprehensive one. An uncapsizable and unsinkable ship must have been the object of naval architects ever since Noah's Ark. If we could only get a squadron of uncapsizable and unsinkable ships and fight an action against a squadron of capsizable and sinkable ships, the issue would not be very long in doubt. Turning to the main features of the proposed design, it will be remembered that the Russians have always had a hankering after great beam. You will all remember the *Popoffka* and the *Livadia*, whose splendid behaviour in the Bay of Biscay was alluded to in the paper. Now, there is an old saying that the proof of the pudding is in the eating, and if the *Livadia* had been such a great success as a sea-going ship, I think it can scarcely be doubted that we should have had some more *Livadias;* but, as a matter of fact, as far as I know, she never has been repeated. It will be noticed (in Figs. 1 and 6, Plate I.) that one of the principal devices adopted in order to make this ship uncapsizable and unsinkable is what the author describes as a treble broad longitudinal side corridor, 18 ft. wide at the side, but this only protects the sides of the ship, and does not go under the bottom. As a matter of fact, in the last naval war (in the Far East) several battleships were sunk by ground mines, and I do not think that any were sunk (although there was one slightly disabled) by a torpedo fired at the side, where this corridor would be a protection. It would be interesting to know how General Goulaeff arrives at the 18 ft. as a safe distance

* In the year 1894 the Minister of Marine included in the French Navy Estimates one ironclad of only 8,000 tons.

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from a high explosive mine. Perhaps he has tried experiments in this connection. It will be seen from Fig. 1 that the bottom of this wide ship is only protected by the ordinary double bottom, and in consequence of her extreme width she will then have about double the chance of picking up a ground mine that an ordinary ship would have, so I do not quite see how she would be unsinkable, though she might be uncapsizable.

With regard to speed, there is no doubt, I think, that you can, with a very broad shallow ship, get great speed. Your wetted surface is not very much greater, and your displacement is not greater, and it has been proved by Mr. Yarrow, in his "River" boats, that with a very broad flat vessel you can get a high speed. But it is only a smooth-water speed. The moment the ship gets into anything but smooth water she loses her speed at once, one reason, of course, being that she pitches her screws out of water; so that I cannot believe that a ship of that form would maintain her speed in anything except very smooth water.

The application of this particular form is to be made to merchant ships also, but I do not propose to allude to this branch of the subject. Steadiness of gun platform is, no doubt, one of the first requisites in a ship of war. It will be seen on page 7, Table II., that up to a wave length of 500 ft. a ship of this type rolls through a greater arc than a ship of ordinary form. The typical ship given for comparison with the Goulaeff ship is the *Revenge*. With a wave of 10 seconds period and 513 ft. length, the arc of rolling is practically the same, but even then the velocity of rolling is very much greater in the Goulaeff ship than in the *Revenge*, and if you come down to a wave length of 185 ft.—which is more than you meet with under ordinary circumstances—the arc of the Goulaeff ship is nearly double that of the *Revenge*, and the angular velocity of the oscillations is as 6 to 1·3. That means an exceedingly quick, jerky roll, which would render accurate shooting quite out of the question as compared with the *Revenge*, so that I can scarcely think this Goulaeff ship would be a steady gun platform under ordinary conditions. There is one point on which we must all congratulate General Goulaeff, and that is the frank and open manner in which he has put his invention before this Institution and asked for criticisms on it. It is a rule, I think, in all discoveries in science and art that when a man invents anything new he publishes it for the benefit of mankind in general, and, although it has lately occasionally been departed from in Naval Architecture, I think it is a good rule, and we must all congratulate General Goulaeff on having put his views so frankly before us and asked for our criticisms.

Professor J. H. Biles, LL.D. (Vice-President): My Lord and Gentlemen, this interesting paper by General Goulaeff opens up a wide subject; the idea of increasing the breadth of ships and taking advantage of that increased breadth is one that has pervaded this Institution for a great many years. I think one may say that at the present time the general tendency of the forms of boilers and of machinery is such that we want area rather than volume. We have small water-tube boilers; we have turbines which occupy floor-space rather than depth; so that what we look for is rather area for putting things into the ship than depth, and this makes a shallow ship, for that reason, desirable. This raises the question of whether it is to be a broad shallow ship or a long shallow ship. Now, the limitation of strength comes in and prevents the vessel being long and shallow on account of the extra material that is required to make up the strength; so that one is, to some extent, forced to a broad shallow ship to get the maximum advantage out of the design which will give the best results on paper. We are, therefore, fully justified in very carefully considering this question. The two objections to the broad shallow ship have been well stated by General Goulaeff. One is the objection on the score of supposed extra resistance, and the other is the objection on the score of rolling. Admiral FitzGerald has dealt with the rolling to some extent. We may say, generally, even from General Goulaeff's figures, that within the limits of size of waves that are common at sea, his own figures show that a broad ship, such as he proposes, is not as good a sea boat, either for passenger work or for warship work, as the narrower ships with smaller stability. Of course, when one gets to waves of above 500 ft. in length, the advantage of the great beam is apparent. Therefore, we have to consider what is the probability of meeting with waves that are over 500 ft. long as compared with waves that are under 500 ft., and the effect of that probability upon passengers and

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upon sea-fights. Judging from my own experience of travelling, I should be disposed to say that there is a great deal more weather that might be characterised as being associated with waves of under 500 ft. than with waves of over 500 ft., and I should prefer, as a passenger, to be in a ship that was suited to the former rather than the latter. I do not see, therefore, that there is any advantage to the passenger in having the greater breadth of ship, for a sea-going ship. When we come to warships, one has to consider what is the probability of an action being fought in weather characterised by waves of over 500 ft. I am not familiar with the history of all the naval actions; the only one bearing on this point that occurs to my mind at the moment is the one which was fought in or outside Quiberon Bay, in which Admiral Hawke chased the enemy in a gale of wind and carried the action through in that weather. But my impression, from general reading, is that actions have generally been fought in weather that certainly would be characterised by waves of much less than 500 ft. Therefore, we should be much more likely to find a broad ship at a disadvantage than a narrow ship, so far as weather is concerned.

Now I wish to say a word with regard to the subject of resistances. A new feature to me, in this paper, is that the resistance of the broad shallow ship is so much less than that of the ordinary type of ship. In connection with that, I should like to say that Mr. Archibald Denny, who, on account of his health, has been unable to come here to-day, has sent me some notes on the resistances of forms, which bear upon this question. I am sorry that the dimensions of the ship were not given in the paper, because it is a little like groping in the dark to deal with forms which are only stated in displacement and whose dimensions are not given, but I suppose I may take it that the diagram on Fig. 1 is approximately to scale ?

Mr. TENNYSON: Yes.

Professor BILES: That enables one to take the form as it is and to measure the ratio of the dimensions and find from the displacement given what the dimensions really are. Now, I take it that the dimensions of the ship shown on the drawing are about 460 ft. × 112 ft. to 114 ft. × about 22½ ft. draught.

Mr. TENNYSON: Those are approximately right.

Professor BILES: Upon that, Mr. Denny, who has taken a little trouble in the matter, says that from the results of his tank experiments on such forms it is not difficult for them to arrive at the resistance of such a form,* and he agrees closely with the lower part of the curve in Fig. 3 (Plate I.); but his estimate is somewhat lower than the curve shown in Fig. 3 from 17 to 20 knots. He also points out that the curve B, which is given for comparison in Fig. 3, is not by any means the best curve of the type, and that, instead of the curve B being above the curve G (General Goulaeff's form), the curve of a ship of the best form would be much below the curve G, so that the resistances which are shown as appertaining to a ship of the ordinary form, such as the *Borodino*, marked "B," compared with the curve of the Goulaeff form, marked "G," are not really representative of the best comparison that can be made between the two forms.

With reference to Fig. 4, curve A (which represents the *Askold* type) agrees with Mr. Denny's results, but curve G he thinks is too low, unless that form is something very much better than in Fig. 3. We have already the form (460 ft. × 115 ft. × 22 ft. 6 in.), so that this, I take it, fixes the type of the Goulaeff form. Passing from that type, which is 17,220 tons displacement, to the displacement given in Fig. 4, which is 13,180 tons, we have a vessel which, if the same proportions are maintained, is 422 ft. long by 103 ft. wide and 20 ft. 7 in. in draught. If those are the dimensions, approximately, which are adopted, then the curve of the Goulaeff form is too low in Fig. 4. Coming to Fig. 5, which is a 23,000 tons displacement ship, Mr. Denny thinks he is on very much safer ground there, because the *Merkara* is quoted, and the *Merkara* was built by Messrs. Denny, so that they ought to have some idea of what is the resistance or effective horse-power of their own form. He says that the dimensions of such a vessel

* See footnote, page 23.

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of 23,000 tons displacement and of the *Merkara* form, work out at 665 ft. long, 67 ft. beam, 28 ft. draught, and block co-efficient of nearly ·66. The effective horse-power of such a vessel, at 24 knots, is fully 29,000 instead of 22,500, as shown by the curve F. A comparison, of course, can be made between the resistances of these forms on certain assumptions. The assumptions that Mr. Denny has made are quite reasonable, I think, although they may not be exactly true to the facts, but the results will not be very different. As a net result of these calculations, considerable changes would have to be introduced in order to make the results of Mr. Denny's calculations agree with those of General Goulaeff. The effective horse-power at 28 knots is 65,000, as against 35,000 for the Goulaeff form; and at 26 knots 40,000 as against 27,000. So that we have there some very considerable differences—I will not say, of opinion, but, at any rate, of results of calculations—which shows that we cannot accept right off the results which General Goulaeff puts forward as being the resistance or effective horse-power of these ships. There is another point which I think is a little misleading. You have seen these alarming diagrams in Fig. 6 of battleships loaded to considerable angles of heel. These ordinary battleships, which are heeled over to 30 degs., as compared with the 3 or 4 degs. of the Goulaeff type, are really not directly comparable, because the 18 ft. safety zone marked off by the three longitudinal bulkheads shown is quite as applicable to the present type of ships as it is to the Goulaeff type, and if you were to cut off from the present type of ship a zone 18 ft. inwards from the side and then show only the flooding of that zone, you would not find a very great difference in the inclination of those two types.

There is another point, of a general character, about which I should like to say a word. It seems to me that if you want to provide against transverse inclination, there should be no longitudinal bulkheads of any kind. If you want to keep a ship upright, the natural thing to do is to put in as many transverse bulkheads as you can, and, if any of the compartments are damaged, let the water flow freely from one side to the other. If that is admitted, then the advantage of extreme breadth very largely disappears. The behaviour of the ship at sea I do not care to say very much about after what Admiral FitzGerald has said, but while he was speaking it occurred to me that the proportions of such a ship as is proposed by General Goulaeff for passenger service, drawing 18½ ft. on 23,000 tons displacement (the same displacement as the *Deutschland*), would not be very far from the proportions of the *Livadia*, at any rate, in the relation between length and draught, and the objection that has been pointed out as being the real cause of the failure, if one may call it so, of the *Livadia*—namely, the smallness of the draught—would be repeated, not perhaps to such a great extent, but, at any rate, in character, in a type of ship which would have a draught of 18½ ft. and a displacement of 23,000 tons.

Mr. W. H. Whiting (Member of Council): My Lord Glasgow and Gentlemen, this paper of General Goulaeff's is, if I may say so, characteristically Russian in its boldness of conception and in the determination to follow out the idea to its logical conclusion. We recollect how Admiral Popoff, thirty-five or forty years ago, set himself to cope with the difficulties caused by the ever-increasing weight of armour and armament, and invented the Popoffka type of warship, and how, in the same way, Admiral Makaroff—a very brave and resourceful officer, whose loss we all deplore—set himself to prevent destruction of ships by collision. So now, General Goulaeff brings before us a plan with which we may not all agree, but which is admirable in its boldness. We are at least bound to agree that any plan which holds out a promise of guarding our great battleships against terrible disasters such as occurred in the recent naval war, is one worthy of consideration.

In the first place, I should like to point out that the statements made in the opening paragraph are not really true as regards British warships. The author there states that "it has at last been recognised that as soon as a ship of the present ordinary form, proportions, and system of construction receives a blow either by ram, torpedo, shot, or collision, perforating the under-water part of the hull and causing more or less serious injuries, the vessel immediately heels dangerously over and loses her stability." And he goes on to show how it is nearly always accompanied by "a considerable longitudinal trim, and (the vessel) gets depressed either by the bow or stern immersing part of her upper structures. . . . By these means the vessel gradually and very

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rapidly loses whatever stability she might have had originally, and finally upsets or capsizes." I wish to say, in the plainest possible terms, that that statement is not true of any British battleship of the present day, with which I am acquainted. Risks, in action, of course, they undoubtedly run—grave risks, serious risks—but, fortunately, not such a risk as that. The examples which are referred to in the first paragraph are, I think, distinctly misleading ones. The *Victoria* is not, in this connection, to be taken as a representative of warships of the present day; and, moreover, we know that there were special reasons to account for her loss connected with the failure to close certain watertight doors. Then, with regard to the sudden loss of some of the vessels in the Russo-Japanese war, including the *Petropavlovsk*, *Hatsuse*, and *Mikasa*, it must be remembered that these disasters were caused either by mines whose size we do not know, or by the explosion of their own magazines. If a ship's own magazine explodes, or if two large mines in succession strike her, we may well despair of her survival. As regards the first and most conspicuous of the losses in the battle of Tsushima, it cannot be taken for granted that the *Osliabia* was a good specimen of modern battleships. In the case of the other three vessels—the *Borodino*, the *Alexander III.*, and the *Suwaroff*—so far from their disappearing suddenly, they stood very severe gun-fire for some hours before they succumbed.

The paper covers a good deal of ground, as Professor Biles has observed, and I do not propose to refer to those points which have been dealt with by previous speakers—points as to the speed, the rolling capabilities of a vessel of this kind, and the applicability of the system to merchant ships. I shall merely treat it as a warship design. I have spent a good many years in that sort of work, and I want to make a few comments on the two sketch designs which are before you in Fig. 6, just as if I were endeavouring to develop the upper one into an actual ship. Some little difficulty has been caused by the fact that the dimensions of the vessels are not given clearly in the paper. I came to the conclusion that the dimensions must be very nearly what Professor Biles makes them—460 ft. in length by about 113 ft. in breadth and about 21 ft. in draught. That means that, starting from a vessel of ordinary type, the draught has been reduced from about 27 ft. or 28 ft. to 21 ft., and the breadth has been increased from about 83 ft. to about 113 ft., the displacement remaining constant at 17,220 tons. Now, I ask you to note what follows. The first thing one notices when one looks at the upper series of sections in Fig. 6 is this—that you have abandoned the very cheapest form of protection against gun-fire which you can have—namely, that of putting the important portion of the vessel well under water. You have raised a very large part of it 7 ft., thus bringing it nearer the water-line, where it is much more liable to be struck by projectiles or injured by shells bursting about the water-line. Not only so, but you have great difficulty in maintaining the same thickness of protective deck. With the same height and the same hull scantlings, you must either have a smaller ship, or if, as in the present case, you are to adhere to the displacement of the corresponding vessel of ordinary type, the weight of the structure itself must be increased. A simple illustration will show that this is the case. Take, for instance, a box of square section; if you want to make a box of the same volume, but twice as broad as it is deep, you must have a heavier box. That is true of any structure, and, of all the structures in the world, it is especially true of a ship which has armoured decks, because for every foot of breadth that you add to your armoured deck, you have not only to increase the weight of hull, but to increase proportionately the weight of your horizontal armour. Everyone who has had anything to do with the design of warships must know that this is a most serious and important feature. I should have liked very much to have the weights of these ships put before us, and we should then have known how General Goulaeff has dealt with that difficulty. The general result is, however, certain. If your displacement is constant, and you cannot save anything on the hull structure, but must, on the contrary, add to it, and if the area of your protective deck is at the same time increased, since your vessel is a broader one and a little longer as well, it is perfectly clear that you must save the balance out of your protection in some form or another. You would have to reduce the thickness of your deck or the width or the thickness of your side armour. There are indications on these little sketches, which appear to have been carefully made and drawn to scale, that both of these methods have been employed—in other words, that the defence has been reduced materially.

Then there is another practical drawback to a design of this sort, namely, the great difficulties in the

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allocation of the hold space. As soon as you try to work out the design of a shallow vessel of this sort you find various difficulties in connection with the means of access to the different compartments and the communication between them. As regards the transmission of power by steam pipes or by electric circuits, all such leads are brought nearer to the deck—the very thing which we try to avoid. You have here your arterial and nervous system, if I may so describe it, brought to the front and put in as exposed a position as possible. That is an inevitable consequence of the shallowness of the vessel. In another way, too, the risk of disablement by gun-fire is very greatly increased. The beam is increased as nearly as possible in the proportion of 4 to 3, and the horizontal target in just the same proportion. Unless you are firing at point-blank range, that is a very serious difference, for if you are firing at any range at which the angle of descent is appreciable, the horizontal dimensions of your target come in. That alone would handicap this vessel enormously in an action; in fact, the enemy has a chance against you of, as nearly as possible, 4 to 3, whether you are fighting him on the bow or on the beam. And the situation is not improved by the great slope which is given to the deck. No doubt that great curvature which is given to the deck in this sketch design is due to a desire to save side armour; but, of course, as is always the case, you have to pay for it, and what you pay in this case is that you increase the angle at which a descending shot strikes your deck plating. You convert it from an angle of, say, 5 deg. to perhaps 7 deg. or more. That may make all the difference as regards perforation. Actual experiment shows that small differences of that sort may well make all the difference between safety and destruction. This slope of deck should at least be made the same in the two designs. Then, as regards the possible angles of inclination produced by damage to wing compartments, I agree with Professor Biles that there is not nearly as much gained in this design as is suggested. You do not gain much in regard to heel, and as regards longitudinal trim, you gain very little indeed. In vessels of this kind it is the longitudinal change of trim which is as important a feature as the transverse change, and inasmuch as if you open up one of these spaces towards the end you open up a bigger area of water-line just in proportion to the increased breadth, your change of trim would, on that account, differ very little indeed from that of the vessel of ordinary type.

Another point to notice is that there is no attempt made to guard the vessel against changes of trim by raising the armour towards the end of the ship, or by thickening it. If you look at Fig. 1 you will see there that there is clearly indicated a great thinning of armour towards the ends of the ship; in fact, the ends seem to be protected by very thin armour indeed. On all those grounds, I say, the ship is much more open to gun-attack. Now let us see how far the avowed object of this change is attained. General Goulaeff is really designing this vessel to resist torpedo attack, and he gives some figures—the result, as he says, of calculation. As Professor Biles points out, they are not, strictly speaking, the result of calculation. I should say that they are, on the contrary, the result of assumptions—assumptions which cannot really be properly sustained. You assume in one case that the engine-room is bilged, and in the other case you assume that it is not bilged; but there is no reason why you should assume this in one case and not in the other. In the one case, too, you assume that the torpedo comes just at a particular spot. If, however, in Fig. 6, you suppose that a torpedo strikes, say, 14 or 15 ft. below water, the after boiler-room would be opened right up to the water, and you would have a more serious condition of damage than would be likely to occur in most ordinary battleships. The after boiler-room is one large compartment in which there are twenty-two boilers out of a total of forty installed in the vessel, and you would get into that compartment from 2,000 to 3,000 tons of water.

Then it is worth noticing on Fig. 1, in the section on the extreme left at the top of the page, how very cramped is the after 12-in. magazine, and also how, so far from the magazine being protected by a wide cellular structure, as described in the paper, the corner of the magazine is actually in contact with the bottom of the vessel. The defence which is supposed to be provided by the cellular construction of the wings does not extend to the main after gun-station at all. Admiral FitzGerald and Professor Biles have both called attention to the fact that the ship would be an uneasy gun platform; in fact, this is admitted in the paper. It is stated that the ship will roll uniformly, though rather briskly. There is no doubt, from the figures quoted, that she will roll briskly, and that would be bad for gun-laying. To

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acquire any marked superiority over the ordinary type of vessel, you have to search for a wave of something like a third of a mile in length. I notice (on page 9) that Mr. Tekhel gives the angle of rolling for the *Weissenburg* as 43 deg. It is of secondary interest to know what your ship rolls when the enemy is rolling at an angle of 43 deg.; she probably would not do very much shooting. These diagrams and tables are set out with a high degree of particularity, but my own impression is that they need a much fuller examination before they can be accepted as a basis of so great change in practice. General Goulaeff's proposal starts, in the first place, from an exaggerated idea of the risks which a well-designed modern warship runs, serious as those risks undoubtedly are. It involves great difficulties in the arrangements of the ship, and a reduction in the average efficiency of the defence; it does very little to secure its avowed object—that is, to guard against serious injury by mines and torpedo attack, and consequent loss by capsizing, and it increases in a marked degree the risk of destruction by gun-fire. It is, nevertheless, true that we are very much indebted to General Goulaeff for the boldness and frankness with which he has put this project before us, and for the labour and thought which he has bestowed on a very important subject.

Mr. C. Tennyson (Member): My Lord and Gentlemen, allow me to state that General Goulaeff desired me to express his deep regret at not being able to be present in person to read this paper, and that he requested me to undertake this duty for him, and to represent him during the discussion upon it. I beg to be allowed to thank M. Bertin for his very favourable comments on General Goulaeff's idea, and at the same time, I should like to say a few words in connection with those remarks. M. Bertin pointed out, amongst other things, that in modern warships we have to take into consideration, not only the statical, but also the dynamical stability. General Goulaeff was, I presume, quite aware of this, but the reason why he considered the effects of the torpedo more than that of shell-fire or gun-fire was, as I understand it, that, from a structural point of view, a modern ship does not so much fear gun-fire as she does a torpedo; and, in any case, as in General Goulaeff's ship the statical stability is considerably greater than in ordinary modern ships, any damage by gun-fire below the water-line, so far as it tends to capsize the ship, is therefore less serious in the Goulaeff type. At the same time, openings above the water-line will not so easily be flooded by waves, owing to the different behaviour of these ships amongst waves. Admiral FitzGerald asked why the *Livadia* was never repeated. I may answer that question by saying that the reasons for constructing the *Livadia* were the same as for the construction of the *Popoffkas* (circular ships), and quite different to what General Goulaeff has in view at present. The idea of the *Popoffkas* was only to reduce the weight of the ship's hull and armour, and, as we have seen from what has been said to-day, we have certainly a greater weight of hull and armour in the Goulaeff ship, so that it is, in a sense, an opposite idea to that of the *Popoffkas*. The principal reason why the *Popoffkas* were never repeated was certainly that their speed was and could only be small, and they were quite unmanageable. They had six screws and shafts, and their manœuvring qualities were very bad; whereas the Goulaeff ship, in relation to her principal dimensions, is quite different to the *Livadia* and the *Popoffkas*. For instance, the relation of length to width in the case of the *Popoffkas* was equal to 1, and in the *Livadia* this relation was equal to 1·6. In the Goulaeff type of ship this relation is equal to 4 or 5 and even more, just as in ordinary ships, and this constitutes a great difference.

Admiral FitzGerald also asked why the author had anticipated an 18 ft. radius of action of the torpedoes. I may say that this dimension was arrived at in consequence of the facts which had occurred during the last war. Of course, we have to assume some sort of radius of action if we want to design anything. If anyone proves that this radius of action is not 18 ft., but that it is 19 or 20 ft., or perhaps less, then the ship would be designed differently. Mr. Whiting pointed out that the comparison in Fig. 6 between the heel of an ordinary ship and that of a ship of the Goulaeff type was not quite fair, because in the one case it was anticipated that a very large compartment was flooded—the boiler-room and the engine-room—whereas in the Goulaeff case we had only a wing compartment flooded. Now, that comes from the reason already given. If we assume a certain radius of action for the torpedo—say, 18 ft. —then the design for the Goulaeff ship will provide a wing bulkhead at this distance from the outer shell,

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and only these wing compartments will be flooded, whereas no ordinary ship can have in her principal compartments a wing bulkhead at a distance of 18 ft. from the sides, and the whole engine-room or boiler-room would therefore be flooded. I cannot understand why Admiral FitzGerald expects a ground mine to explode under the flat part of the ship's bottom. This would, I think, be a very rare and exceptional case; in the great majority of cases a ground mine will certainly strike the ship's side before it touches the flat bottom.

As regards the speed, which was also commented upon, I think it is to be regretted that the author of the paper did not give all the dimensions. In reading the paper I gave some of the dimensions of the models which were experimented upon, but I may point out that the sketches and curves given refer to different models of the Goulaeff ship in almost every case; so that no comparative conclusions can be drawn from these sketches or from figures given here. Therefore, the comparisons made by Professor Biles and Mr. Whiting are not quite fair. Moreover, all the data were taken from well-known sources, given in the paper, and from experiments performed officially and very carefully at the St. Petersburg experimental tank.

In connection with the question of gun-firing from the Goulaeff ships, I do not think that angular velocities and angles of rolling have such an important bearing upon this question. I think the method of firing from a ship should always be adapted to the character of the ship's rolling. Taking the angular velocity of rolling in ordinary ships, you will see that in most cases—and this has been proved by calculations founded on the formulæ usually employed—amongst ordinary waves, even far shorter than 500 ft., an ordinary ship will generally have an angular velocity too great to allow the object fired at to be hit, and you will have to wait for moments when, during the rolling, the angular speeds are lower. Now, as we know that the character of the rolling of an ordinary ship amongst waves is very irregular, as is shown by the curve on page 8, and as has been very aptly described before this Institution by Mr. Froude,* you will never know for certain, in practice, when you are firing from an ordinary ship, at what moment the angular velocity will be small enough to enable you to aim accurately at the object. Now, ships of the Goulaeff type have a uniform kind of rolling, and in the case of battleships having a breadth of more than 100 ft., these ships will probably never roll much. I think you will agree that very small waves will not be able to make the Goulaeff ship roll at all, and comparatively big waves will make her roll more or less regularly, so that you will practically have moments—say, every few seconds—when you can aim accurately at the object at which you are firing. If our present system of firing is altered to suit the special character of rolling of the Goulaeff ships, I think we can easily get better results from firing from them than from ordinary ships, because the former will always give you moments when the angle of velocity will almost vanish, and these moments will always come at more or less regular intervals, and with the ship approximately in the same position in each case. I would also like to comment on Mr. Whiting's remark that in this ship, as the draught is less than in ordinary ships, we get more of the vital parts, such as steam pipes, &c., near the water-line, where they may easily be damaged by gun-fire. From the figures given in the paper it is evident, however, that the draught remains so considerable—21 ft. and more—that all the steam pipes, &c., will still remain, in relation to the water-line, where they are in the present ships. In conclusion, I think it would be only right, as I am not the author of this paper, if the remarks made by the distinguished speakers who have been kind enough to comment on the paper were referred to General Goulaeff for his reply. Finally, my Lord and Gentlemen, allow me, on behalf of the author and myself, to express our sincerest thanks for the kindness of this distinguished audience, and the interest with which the paper has met to-day.

The PRESIDENT: I am sure, Gentlemen, it will give us the greatest pleasure to give the author of this paper, General Goulaeff, an opportunity of responding to the remarks which have been made by the speakers here, and I beg to thank him, in your name, for his interesting paper, and also to thank Mr. Tennyson for the very admirable way in which he has answered, as far as he could, the remarks made upon this paper.

* See Transactions I.N.A. for 1896, Vol. XXVII. page 293.

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Written Contributions to the Discussion.

Mr. Lyonel Clark (Member): It is not in my province to discuss the merits of General Goulaeff's new type of ship, but some remarks as to the behaviour of vessels of large beam in a seaway may not be without interest. The floating docks which it is my lot to design are broad, shallow vessels, with a beam of 100 to 140 ft., and a metacentric height of well over 100 ft., and when the towage of such craft in the open ocean first came under consideration, we were somewhat afraid that the great metacentric height would cause violent rolling, with possible danger to the side walls, the bottoms of which are slightly higher than the bottom of the pontoon, and would, if they rolled out of the water, strike the surface heavily when re-entering it again. The first experience, however, that we had showed that this was a fallacy. I find from some notes made at the time that the Havana Dock, on leaving the Tyne, encountered a nasty swell, which caused the towing vessel—one of about 4,000 tons—to roll very badly indeed, but the dock itself was remarkably steady, the maximum roll being computed not to exceed 1½ deg., whilst the periods varied from 6½ to 7 seconds. Since this time we have sent many other and larger docks long distances across the ocean, and in many cases the dock has gone adrift and got into the trough of the sea, but in no case has the rolling been sufficient even to call for remark. I notice also that Commander Bennett* mentions that the Cavite Dock, which has a beam of 135 ft., whilst being towed from the United States to the Philippines, broke adrift in the Mediterranean in a gale which reached a force of 10 by the Beaufort scale, and when in the trough of the sea achieved her maximum recorded roll, which only amounted to 8 deg. It is, therefore, evident that in vessels of great beam, even with excessive metacentric heights, rolling is a negligible quantity.

With regard to the General's remarks as to the ease with which floating docks could be built to accommodate his broad vessels, I can fully bear out his contention. There is not the slightest difficulty in making a floating dock of any desired width, and if, as we may assume, these broad vessels are provided with sufficient docking keels, thus distributing the weight over a large area, such a dock might be of quite economical construction. As regards the price also, the cost of a floating dock, even for an ordinary deep-draught ironclad, is such a small percentage of the value of the ironclad itself that I can quite understand a firm, in a preliminary tender, offering, as General Goulaeff says, to include the provision of a floating dock in the cost of the ironclad itself.

General E. E. Goulaeff (Member): Admiral FitzGerald's main criticism is to the effect that the cellular side corridors only protect the vessel from torpedo attack at the sides, and do not protect her from ground-mines bursting under the bottom. To this remark I may reply that no ship yet designed is quite proof, in the fullest sense of the word, against risk from such mines, and my ship, having all along her lower part the usual double bottom, is therefore protected from ground-mine attack at any point just as much as any other ship. Moreover, being a broad ship of large stability, it is easy to fit her with an additional third bottom over the double bottom shown on the drawings, which cannot be done in an ordinary ship, as the limited stability of all such ships prevents the considerable lifting of weights which is necessary to give room enough for the fitting of the third bottom. Even when only provided with the usual double bottom and fitted with cellular side corridors, as shown in the paper, my ship is better protected than an ordinary one even from ground-mines, as in her forward motion there are more chances that she would first strike such a mine on her side, along a buttock-line,† where the cellular side corridors would come into play and efficiently protect the vessel. The fitting of an additional third bottom is a matter of detail, which only lack of space prevented my alluding to in the paper. Coming to the question of rolling, Admiral FitzGerald, in his criticism on this point, was somewhat misled by the figures given in the tables included in the paper, which represent only the results of theoretical investigations, and not facts derived from practice.

* Journal of the American Society of Naval Architects, Vol. XIX., No. 1.

† This has been proved in the last war by the *Sebastopol* having repeatedly struck such mines exactly along her bilge-keels, which were fitted in a direction coinciding very nearly with the buttock lines.

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The actual rolling at sea of broad ships with large metacentric height, such as the *Popoffkas*, the *Livadia*, the *Henri IV.*, and lastly, the behaviour of floating docks of great breadth, referred to by Mr. Lyonel Clark, prove that if there were any rolling at all with a vessel of my proposed type, it would not be either jerky or heavy. In order to reconcile the practical results of a very wide experience with those given in Table II. of my paper, it would have been necessary to correct the figures in that table by the introduction of several corrections referred to by M. Bertin, and tending to diminish the arc of rolling of my ships. Some of these corrections would involve complications, and could not be calculated exactly just at present. Such corrections, however, would change the whole aspect of the comparison instituted by Admiral FitzGerald on the basis of the figures given in the tables.

As to the accuracy of pointing and shooting the guns mounted upon such vessels, I may state that although the *Livadia*, being designed as a private yacht for the Emperor Alexander II., carried no guns at all, yet the late Sir William Pearce, the builder of this vessel, was so much struck by her extraordinary steadiness in the Bay of Biscay that in this very Institution in the year 1881, he stated* in this connection: "If you are going to fight very big guns, you must, necessarily, have a steady platform, and when you consider that this ship [the *Livadia*] passed through the Bay of Biscay during a week when, it is recorded, more ships foundered than in any week during the whole of last year, you will understand that she must be exceptionally steady, and, as a matter of fact, she rolled only three degrees one way and four degrees another, and the range of pitching was only ten degrees. I am sure that no vessel in our Navy could have passed through that sea without rolling at least twenty-five or thirty degrees, and if a steady platform is of any advantage at all, it is of great advantage for war-vessels intended to fight heavy guns." It is certain, at any rate, that the accuracy of pointing and shooting at sea of the 11 in. and 12 in. guns mounted on board the *Popoffkas* was very satisfactory, and nobody complained about the same in regard to the *Henri IV.* and the old American *Miantonomoh*—vessels which, by their form and proportions, occupy, as it were, an intermediate place between ordinary vessels and those which are proposed by me. I still maintain, therefore, that the big, fast vessels of my type and form, designed either for war or commercial purposes, would behave much better and be more steady at sea than ordinary ships, and this upon the waves of any length and period. To conclude with the remarks of Admiral FitzGerald, he called my ships "smooth-water" ones, as he thought that they would pitch their screws out of water in a heavy sea. But this has certainly not proved to be the case in actual practice, since even *Livadia* (which was only 235 ft. long, drawing about 8 ft., and of about 4,000 tons displacement) maintained her speed against the gale we experienced in the Bay of Biscay in the autumn of 1880 and never pitched her screws out of water. When we think of my ship of some 20,000 tons displacement, having a length of 400 to 500 ft., a draught of more than 20 ft., and of very fine yacht-like entrance and run, we need not have any apprehension on this point. In regard to Admiral FitzGerald's question why the *Livadia* was not repeated, this may be chiefly ascribed to political reasons, the premature death of her much-lamented and august owner, the subsequent death of his brother, the Grand Duke Constantine, who supported the idea of adopting this type of vessel for warships, and, lastly, the death of Admiral Popoff, the originator of this class of ships, which brought into office and power persons who held different opinions, so that there are now left only very few survivors of the great and costly experiment. The results of this experiment ought not to disappear uselessly from the science of naval architecture, now that turbines and water-tube boilers require superficial area rather than volume and depth, and propellers of more moderate diameter within the limits of the shallow draught of the vessels proposed can be used, while the requirements for higher and higher speeds are increasing.

Referring now to the remark made by Professor Biles, in which he states that the curve G (Fig. 4) is too low, "unless that form is something much better than that used in Fig. 3"—that is, better than the form (460 ft. by 115 ft. by 22½ ft.) fixed by Professor Biles himself for the type of my form—I must say that it is not so, as the form, proportions, and dimensions of vessels of my type (as well as those of any other type) are not fixed independently of the

* Trans. I.N.A., Vol. XXII. p. 280.

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destination of the particular vessel. Thus, for the model of an ironclad to be compared with the model of *Borodino* (Fig. 3), the dimensions corresponding to 430 ft. by 108·2 ft. by 21·3 ft. were taken, and those for a specially designed high-speed cruiser to be compared with the cruiser *Ascold* in Fig. 4, a model of more suitable form was adopted, and the dimensions selected were accordingly those corresponding to 498·7 ft. by 85·3 ft. by 21·3 ft. No naval architect would think of giving the same form, proportions, and dimensions to vessels of such different character as an ironclad, a fast cruiser, and a fast passenger steamer. This last consideration fully explains the point raised by Professor Biles with reference to the curves shown on the diagram in Fig. 5, for in this case also the form and proportions adopted for the steamer of my type were certainly not the same as those of the ironclads referred to on the previous diagrams (Figs. 2 and 3), but a different and much better form of my model No. 3 was adopted, as is specially mentioned in Fig. 5—viz., "Curve C, Goulaeff's model No. 3 at the draught 20′ 3″" and "Curve D D¹, at the draught 18′ 3″." I do not know the form, proportions, and dimensions of the steamer of 23,000 tons, with which Messrs. Denny* were experimenting at their tank, with a view to checking for Professor Biles my curves C and D, shown in Fig. 5, but I can assure you that had they taken the dimensions of this steamer of 23,000 tons of my type at 765 ft. by 100 ft. by 20 ft. 3 in.—as I have taken them myself—there would not have been such differences of E.H.P. as Professor Biles was led to believe would exist, and which he gave in his remarks. The results would have been much nearer to those given by me in Fig. 5. As to the difference between 29,000 E.H.P. and 22,500 E.H.P. for the 24 knots of the S.S. *Merkara*, the result of this correction would only be to bring the curve F for this steamer still higher than is shown in Fig. 5, thus proving still more the advantage of my form as compared with that of the *Merkara*. These advantages are so important that I am not surprised that Professor Biles began his remarks on this point by observing that a new feature to him in my paper was that the resistance of the broad, shallow ship is so much less than the resistance of the ordinary type of ship. I am perfectly sure that had the results shown by my curves of E.H.P. on the diagrams Figs. 2, 3, 4, and 5 been checked by experiments in some of the tanks now existing, with models of exactly the same form and dimensions as were adopted by myself, they would have proved to be quite accurate, with a difference of perhaps a few hundred E.H.P. and no more, according to the care bestowed upon these experiments and the precision of the instruments and plant available.

As to Professor Biles' remark that the 18-ft. safety zone marked by the three longitudinal bulkheads shown is as applicable to the present type of ships as it is to the Goulaeff type, I beg to differ from him, as the very limited breadth of even the largest ships of ordinary form would not admit having 18-ft. treble-sided safety cellular zones, as this would involve subtracting 36 ft. out of the total breadth of the vessel, leaving so little room in the hold that there would be no possibility of accommodating the propelling machinery, the coal or other fuel, or the necessary ammunition, not to mention the auxiliary machinery and stores and other numberless things. As to the multiple transverse subdivision proposed by Professor Biles, and considered by him to answer as well as the side cellular zones, the drawing of the hold of an ordinary ship given in Fig. 6 shows this very form of transverse subdivision, which is carried out as far as practically possible. Indeed, this particular ship has as many as eighteen or nineteen complete transverse W.T. bulkheads, with the result that she inclines to 30° as compared with an inclination of 3° or 4° for the vessel of my type. Besides, the destruction of or injuries to the hull produced by modern torpedoes in actual naval warfare represent, as seen in photographs of damaged ships, an ellipse largely elongated along its horizontal axis, so that the water, rushing through such openings, will at once flood, not one, but several of these narrow transverse sections. As to the smallness of the draught (18 ft. 3 in.) of the steamer of 23,000 tons, noticed by Professor Biles, I must say that this reduced draught is in the *light condition* of the vessel corresponding to a displacement of some 20,700 tons, whilst the draught of water corresponding to the *normal* displacement of 23,000 tons amounts to 20 ft. 3 in., and the proportions of this steamer are those which I gave already—viz., 765 ft. by 100 ft. by 20 ft. 3 in.

* We are asked by Gen. Goulaeff to add that, in a letter since received from Mr. A. Denny, it was stated that Messrs. Denny did not make a special model of the vessel illustrated on Plate I.– Ed. Trans. I.N.A.

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Finally, to answer the remarks made by Mr. Whiting, part of which have already been dealt with by Mr. Tennyson, I would point out that, although it is only natural that a representative of the Admiralty should not easily agree, British warships, splendid though they be, are, under certain circumstances, sinkable and capsizable. Certainly, the *Victoria* is not the *Dreadnought*, and is not a representative ship of the British Navy of to-day; neither was the *Captain*, which also capsized. Yet I do not know that British ships possess any special arrangements differing from those adopted in other navies to protect them from torpedoes and collisions. That this is so is proved by the recent *Gladiator* disaster, which could have been prevented by the adoption of vessels of my proposed type and form. As in actual warfare the injuries received by vessels are much more frequent and serious than those produced by mere collision in time of peace, I venture to insist that it is high time to think about adopting some more effective means to prevent such disasters. Mr. Whiting himself does admit some of these dangers when he says: "Risks in action they undoubtedly run—grave risks, serious risks." It is quite true, as he said, that the *Borodino*, the *Alexander III.*, and the *Suvaroff* were far from disappearing suddenly, and they stood very severe gun-fire for some hours before they succumbed. But still, these ships did upset or capsize, showing the imperative necessity of providing some more efficient protection in the ships which are now being designed or laid down.

Answering the latter part of Mr. Whiting's speech, in which he spoke to some minor points of my design, I may say that the arrangement for protecting the armament and the propelling and other machinery from the enemy's fire, notwithstanding the reduced draught—all belong to matters of detail and the requirements to be satisfied in each particular design. It is sufficient to state that an addition of a few inches to the comparatively small draught of water of my shallow broad ship at once gives an additional displacement corresponding to so many inches of deck armour. Even admitting that Mr. Whiting may perhaps be right in the two following points—viz., (1) in the increased surface of horizontal target, and (2) in the additional weight of hull—I consider that these are only minor objections or sacrifices, in exchange for which, by adopting my proposed form and system of construction, we gain advantages of the utmost importance. No one has ever invented anything which gave enormous advantages without some little drawbacks or sacrifices. Even the two last-mentioned objections may be reduced to a minimum by the talent and ability of the designer of each particular ship of the proposed type.

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MODERN ARMOUR AND ITS ATTACK.

By Captain T. J. Tresidder, C.M.G.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 8, 1908; the Right Hon. the Earl of Glasgow, G.C.M.G., LL.D., President, in the Chair.]

The invitation of the Council of this Institution to prepare a paper on the above subject was not accepted by the writer without considerable hesitation, in view of the fact that the material at disposal from most of the possible standpoints was already pretty well used up. A very able paper by Mr. C. E. Ellis, read before the Institution in 1894, brought the history of armour up to the commencement of the period of Harveyed plates; and, three years later, an elaborate treatise was published in Italy in the *Rivista Marittima*. It is true that the present type of plate was then too young to be included, but several papers have appeared in this country and elsewhere in recent years in which the historical and manufacturing sides of the subject have been more or less exhaustively dealt with and brought up to date.

One point of view, however, appeared still to afford room for an article, namely, the theoretical and analytical; so that has been adopted for the present notes, which, it will be understood, are a mere record of individual opinions based on an experience of much less extent than that of some who may read them.

In looking over some old reports of armour-plate trials, the writer was struck by the frequent repetition of "No penetration" in the column headed "Effect on the Plate." This, of course, meant "No perforation"; but the phrase, seeing that it was used not by tyros but by gunnery experts, suggested that, even in a paper intended for submission to a body of scientific men, it might not be out of place to emphasise the necessity for attaching precise meanings to the few words constituting the special vocabulary of ballistics.

When two bodies come into collision, "no penetration" is as impossible as perpetual motion. The hardest plate made cannot arrest a drifting snow-flake without suffering penetration; for the flake has "work"—in however minute a quantity—stored up in it, and to stop it an equal amount of work must be done by the plate.

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What are the agencies the plate can command for doing this work? First, there is F, the mean pressure of the reaction it exerts on the flake.[1] But pressure alone, even if gigantic, can do no work: it must have distance to act through; and, since the whole plate cannot be expected to move under the circumstances, this necessary distance S can only be obtained by penetration. F S represents the work the plate must do, and F S must equal the work stored in the moving flake, which, though ridiculously small, is not zero. Consequently F S cannot be zero, as would be the case if either F or S separately were zero; which proves the statement.

The word penetration must not be taken to have only the limited meaning of permanent penetration. If penetration does not strain the material of a plate beyond its elastic limit it will be temporary only. It may leave no traces; but, if there has been a collision of any kind, it will have occurred all the same. Penetration is incomplete perforation; perforation is complete penetration. The reference to the snow-flake and armour-plate is redeemed from triviality by the fact that it forms a striking illustration of the fundamental principle of armour resistance.

As the weapon of the projectile is $W V^2$, so that of the plate is F S. (See Footnote No. 2). It has no other; and all the variations that are possible in armour resistance are variations of these two factors, although, for any given value of F, considerable variation is also possible in the amount of *maximum* pressure and the instant of its development.

F, being the mean mutual pressure between projectile and plate, can only increase with the energy of the former up to the maximum capacity of the material of the latter. The stronger that material is the larger can F S be (and, therefore, the higher the amount of $W V^2$ that can be resisted) for a fixed value of S. On the other hand, the less rigid the plate is, and the more deformation it can undergo without rupture, the greater can F S be (and, therefore, the higher the amount of $W V^2$ that can be withstood) for a fixed value of F. This constitutes the general principle of which we may now proceed to examine the details.

The resistance of armour plates to perforation by projectiles may be divided into three classes :—

(*a*) Rigid and concentrated.
(*b*) Yielding and distributed.
(*c*) Combination of (*a*) and (*b*).

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[1] The Symbols used throughout this Paper and its Appendices have the following meaning :—

W = weight of projectile in lbs.
D = its calibre in inches.
L = its length in inches.
F = the mean value over the distance of arrest of the end-on pressure in tons.
t = thickness of plate in inches.
V = striking velocity in feet per second.
S = distance in which projectile is brought to rest in inches.

[2] This is a law which governs all plates alike, from the impossible plate of infinite hardness whose F is infinity and S zero, to the impossible plate of infinite softness whose F is zero and S infinity.

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In class (*a*) the molecules of the plate very strongly resist alteration of their relative position, but are incapable of retaining their inter-cohesion when that relative position is forcibly changed. Steel plates that are hard throughout are examples of this class. They present a high resistance per unit of area actually attacked, but do not distribute the work of resistance to perforation[3] much beyond that area. Their F S relies principally on the F element.

Plates that are tough throughout are examples of class (*b*). These offer less resistance per unit of area actually attacked, but utilise very largely the resistance of the metal outside that area. In this class of material the molecules less strongly resist alteration of their relative positions, but are capable of retaining their inter-cohesion when that relative position is forcibly changed to a substantial extent. Their F S relies largely on the S element. The essential difference between classes (*a*) and (*b*) is not one of strength, nor necessarily one of hardness, but one of structure. Class (*a*) materials must have a crystalline structure; those belonging to class (*b*) should have an amorphous one.

Consider the case of two plates of equal thickness, one a sample of class (*a*) having a breaking stress of 100 tons per square inch, and the other of class (*b*) with 50 tons per square inch; and suppose both to be incapable of cracking and to be normally attacked by the same unbreakable projectile at equal increasing velocities. Which will suffer perforation earliest? Probably the hard plate; for, although it can muster 100 tons per unit called upon, it calls upon so few units that the total force exerted by the shell divided by this number may exceed 100 tons before the same force divided by the larger number of units called upon by the soft plate exceeds 50 tons. Moreover, all the work stored up in an unbreakable projectile has to be done on the plate, and must be balanced by the sum of the forces necessary to displace each displaced molecule of plate-metal multiplied by the distance through which each force acts. Each molecule of the hard plate may take a double force to displace it, but the bulk of the force only acts through the distance necessary to produce rupture, and this will be quite a small distance with a hard plate, and a substantial distance with a soft one.

The best illustration of this principle is seen by the stopping of a cricket ball by a net. If, instead of a net, a fence of ½ in. pine were used to stop the ball, it is not impossible that a fast bowler might smash through it. Yet, comparing the materials, one would expect much more strength in ½ in. wood than in netting. The wood fails because it tries to stop the ball with very small self-displacement; its

[3] Resistance to "racking" is altogether a different thing. It used to be as important as resistance to perforation, but improvements in armour have rendered plates practically secure from failure under "racking" attack, so it need not be taken into account in the discussion of armour resistance proper. It must not, however, be lost sight of in the general design of armoured structures, for it may cause failure of the bolts, &c. The more a plate resists with its F factor the more will the bolts be strained in tension; the more reliance is placed on the S factor the more important is it that the bolts should be capable of suffering sudden bending and distortion without breaking.

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F S is nearly all F. The net succeeds because it accepts a large self-displacement; S plays the leading part in its F S.

So far, on the assumption that the projectile is unbreakable, the balance of advantages seems to lie with the soft plate, apart from the fact that it is not liable to crack, which the all-hard plate is. There are, however, one or two considerations which introduce a practical modification into this comparison. One is that it is not always convenient, and sometimes it is not possible, to permit the substantial deformation the capability of undergoing which, without rupture, is the feature of the soft plate. A more or less rigid resistance is essential, for instance, in the protective armour of many gun positions where quite a small deformation would jam the mounting and put the gun out of action. Under such circumstances an all-hard plate might find employment if we could make one that would not crack. Another modifying consideration is that the distribution of work and the material displacement on which class (*b*) relies take time, and, therefore, are less pronounced advantages when the projectile's energy is principally due to high velocity than when it is more the result of large weight.[4]

Modern face-hardened plates come under class (*c*). The hard layer adds to the general rigidity, but while by so doing it diminishes S, it is doubtful if it adds anything to F, although it very greatly increases the pressure at the first instant of impact when the point of the shell, if uncapped, is without lateral support. It alters the shape of the pressure diagram, so to speak, without adding anything to its area. (See Figs. 12, 14, and 15, Plate III.) As the hard layer does not involve liability to cracking of the whole plate, the hard-faced plate (even against unbreakable shells) has advantages for shields whose substantial deformation would be fatal. Seeing that class (*b*) plates, however, can be made to combine high values of both F and S, there would probably be but little scope for hard-faced plates if projectiles could not be broken; but against breakable shells they have a great advantage in the fact above alluded to that they introduce high stress-intensity at the first moment of impact when the shell's delicate point, if uncapped, is very weak. High stress-intensity that is reached at any later stage of impact does not find the point without lateral support, which is the reason it is so important that, in a plate designed to break projectiles, there should be no soft layer whatever in front of the hard one.[5] It was owing to their power of breaking projectiles that hard-faced plates held the field before the general introduction of caps; and, although since that time they continue to hold the field, it is principally to impose upon an enemy the inconvenience and expense of capping all his A.P. shell.

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[4] A 2·24 in. shell of 6 lbs. at 3,000 f.s. and a 12 in. shell of 850 lbs. at 583 f.s. have equal energy per inch circumference. A hard-faced thin plate would give good results against the former (although small uncapped projectiles are less easily pulverised on a hard face than large ones), but would make a very poor show against the latter. With a strong, tough, homogeneous plate, erected so that it could accept deformation, the difference would be just the other way.

[5] A case is remembered of a treated compound plate where the face had good, punch-breaking hardness about $\frac{1}{16}$ in. below the surface, which was itself soft. It was perforated by projectiles it would certainly have defeated if the thin soft layer (which helped the shell like a cap) had been removed.

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Caps[6] play such an important part in the present-day attack of armour that it is desirable to devote some space to their discussion. Let us first consider the manner in which an uncapped pointed projectile behaves on impact with a hard face, and then pass on to discuss the purpose a cap serves, and how it serves it.

At the moment of first impact of an uncapped pointed projectile of given weight and calibre on a hard face, an "end-on" pressure, dependent directly on the square of the velocity and inversely on the amount of yielding of the plate, is applied to its point. This pressure it will be strong enough to bear if V does not exceed a certain value, which may be called the first critical velocity. Up to this value of V the projectile needs no cap, because it is strong enough without one. All the work is done on[7] the plate, which receives an indent almost as if it had no hard face; this indent may amount to perforation if the plate is thin enough and well held up. The projectile should remain intact.

Immediately V is increased beyond the first critical value (though it may be by the addition of only a single foot-second) matters undergo a radical change. The initial pressure reaches an amount in excess of the maximum that can be supported without fracture by the projectile's point, which consequently fails and involves the destruction of the whole shell. This, in flying to pieces, absorbs so large a portion of the whole work that it leaves but little to be done on the plate; so the latter suffers to a very small extent, and very much less than at the preceding lower velocity.

As the speed of attack is increased still further the wrecking of the projectile is more and more complete, and the amount of work absorbed by it increases also, but by no means in proportion to the increased total. More and more remains over, therefore, to be done on the plate, which accordingly suffers more and more till at last perforation is effected. This perforation is not in the form of a clean parallel hole, but a rough conical one of great size at the back, from which a large cone-shaped disc is smashed out.

The augmentation of velocity being still continued, another critical velocity—which may be called the third, and is an extremely high one—is eventually reached, when the projectile, though uncapped, is able to stand the initial compression stress, either because of the extra rigidity imparted by inertia to its molecules, or

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[6] The idea of putting a cap on the point of a projectile to save it from fracture on impact with a hard face emanated originally from a British Engineer Officer, Captain (now Lt.-Colonel) English, in 1878. It was suggested by the behaviour of a Palliser shot that had been fired against a compound plate accidentally erected with its soft side in front. Captain English designed the first cap that was ever tried, and it gave a successful result, but the matter was allowed to drop for some reason that has not officially transpired. The Russians took the idea up in 1894, and since that time the use of caps has extended and is now almost universal for projectiles intended to perforate hard-faced armour.

[7] The difference between "on" and "by" is important. All the work stored in a projectile must be done *by* the plate that stops it; but the amount done *on* the plate will be less than the whole by the quantity, if any, that is done on the projectile.

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because of this combined with an actual reduction of pressure.[8] It then goes through unbroken and leaves a clean hole.

The nature of the uncapped projectile's failure between the first and third critical velocities may be confidently stated to be the following :—A small piece of the extreme point in the form of a double-ended cone is driven back into the head and splits it like a wedge (Fig. 1, Plate III.). This initial split is succeeded by numerous others following each other in the order of the numbers in Fig. 2, maintaining a direction approximately parallel to the rear surface of the originally formed double cone. The idea is that when the point is arrested it causes a surface of cleavage say at 1, 1 (Fig. 2); nevertheless, the point enters the plate and the shell is again arrested as at the points 2, 2, 3, 3, and so on, each time cleaving along a new conical surface, extremely near the previous one at first, and a little more widely spaced later on. The resulting conical laminæ thus initiated break up and fly tangentially to the plate as fast as they form, so that the rear part of the shell is "piled up" on the front part as in Fig. 3 which was sketched from an actual result.

It will be observed that the mischief originates close to the point, and that the fatal first wedge does its work by bursting laterally the portion of the head just behind it. If this portion were fitted with a reinforcing ring of adequate strength (Fig. 4), the wedge could not be driven back, and the point would be saved. A larger wedge, shown by dotted lines, would then try to burst the head further back, and another reinforcing ring would be required (Fig. 5). But the projectile is larger and stronger here, so the second ring need not be so strong as the first. Imagine this process repeated, of larger and larger wedges counteracted by weaker and weaker rings, till a part of the head is reached where there is mass and strength enough to need no reinforcement. The system of rings is then like Fig. 6, and, if all the separate rings are united into one wide ring of varying thickness, the dotted line of Fig. 7 results, which shows a section of a Firth cap after being experimentally crushed to the level of the shell's point by a single blow of 26·5 foot tons. From this it will be seen that the main function of the cap is that of a series of reinforcing rings, whose thickness, and therefore strength, should be proportioned to the amount of reinforcement needed at each part. Figs. 8 to 11 (Plate III.) show various other 6-in. caps before and after being crushed by a single blow of energy proportioned to the cap's design.

Postponing for a moment the consideration of the design of the cap, it is important to note how its efficiency depends on the projectile having adequate velocity. Three critical velocities have been mentioned; the first being that which raises initial intensity of compression stress above the limit the uncapped point can stand, and the third that which, either by reducing initial pressure or by raising the strength of the shell, or both, enables the uncapped point to bear the compression stress it is subjected to. (A tallow candle, driven undeformed through a wooden board, is an

[8] The writer has a theory on this subject which he had intended for inclusion in these notes, but the intricacy of the argument rendered it impossible for a reasonably brief summary to do justice to it.

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excellent illustration of the third critical velocity). Below the first and above the third critical velocity the projectile needs no cap, because it is strong enough without one. The support afforded by the cap obviously depends on its resistance to stretching in the time available. The shorter that time the more power will be needed to make the cap stretch within the limit of it. The time concerned is inversely proportional to the striking velocity. Therefore the greater V is the more reinforcement of lateral strength will the cap afford to the projectile; and, inversely, the smaller V is the less efficient will the cap be. It is easy to see, then, that there must be a velocity which is only just high enough to enable the cap to give adequate reinforcement. This may be called the second critical velocity.

Recapitulating, we may state that, in the attack of a hard-faced plate with increasing velocities,

The first critical velocity is the minimum value of V which raises initial intensity of compression stress beyond the limit the point of the uncapped projectile can bear without fracture.[9]

The second critical velocity is the minimum value of V which enables the cap to adequately reinforce the projectile's point so that it can bear a higher initial stress without fracture.[9]

The third critical velocity (much higher than the other two) is the minimum value of V which enables the point of the uncapped projectile to again bear without fracture the stress-intensity it is subjected to.

The third critical velocity undoubtedly exists, as otherwise a soft candle could not be driven undeformed through a wooden board,[10] the explanation of which phenomenon affords room for much interesting discussion. If there is nothing the matter with the plate this velocity may be expected to be a very high one.

The second critical velocity appears to lie, according to quality of plate and quality and design of projectile and cap, between 1,650 and 1,800 f.s.

The first critical velocity has been noted as high as 1,620 f.s. with a 4·7-in. Krupp shell and a 6-in. Harveyed plate; but against a modern Krupp plate with a proper depth of hardness, it would probably be very much lower. If, then, a capped shell succeeds in perforating unbroken at 1,600 f.s., or less, it does not follow, as is often argued, that that velocity is high enough to make the cap efficient; for, with an

[9] The first and second critical velocities may in some cases overlap so that there may be no velocity high enough to break the uncapped projectile and yet at the same time low enough to render the cap inefficient. In such cases a capped projectile is saved from fracture of its point at all velocities. In other cases (dependent on quality of projectile, plate, and cap) there may be a small range of velocities that are too high for the uncapped projectile to stand without fracture, and at the same time too low to render the cap efficient. In such cases the projectile will fail to hold together over this range of velocities whether capped or uncapped.

[10] This is not merely a reference to an old legend that all have heard of but few have verified. Among the numerous experiments carried out for the purposes of this paper the "candle and board" legend was put to the test, and it was found that marked decrease of deformation in the candle attended moderate increases of velocity.

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inferior plate, or a superior shell, it may be that it is low enough to make the cap unnecessary.

We may now consider the design of the cap and its effect upon results.

Up till recently the writer held the opinion that the lateral reinforcing action of the cap, already referred to, was the only action of importance it performed. This opinion, however, he has now modified after further detailed study of the problem, very much assisted by the working out of stress diagrams, of which Appendix B contains some examples. He still thinks that this reinforcing action, if in large quantity (as in the case of a substitute for a cap consisting of a large W.I. plate placed in front of a hard face), would not need supplementing in any way; but it is impossible to deny that it is supplemented in fact, whether it needs it or not.

When the cap and plate first come into collision a high stress-intensity occurs for both. Does the cap—(1) yield to this by being flattened as in Figs. 7 to 11 (Plate III.) before the hard face of the plate is disintegrated; or (2) does inertia so support it (as in the case of the candle experiment) that it pierces the hard face without being deformed itself; or (3) do deformation of the cap and disintegration of the hard face go on simultaneously? The probably correct answer is that (1) occurs up to a certain velocity; then (3) as velocity increases; and finally (2) when velocity becomes very high indeed.

If the lowest velocity at which (3) begins exceeds practical fighting velocities, then the cap should be designed as a lateral reinforcer only. That is, it should have, to begin with, one of the forms of the crushed caps shown in Figs. 7 to 11, because it is wasteful of the shell's energy to call upon it to do this preliminary flattening which might better be done in the factory. More than this, the extra resistance to stretching from a state of rest the cap has in virtue of the inertia of its molecules is lost if the crushing is done against the plate, as just when their reinforcing action is called for these molecules are already in rapid motion in the direction stretching gives them.

If, on the other hand, practical fighting velocities enable the cap to act as in (2), or even as in (3), then it should be sharp-pointed in order to ensure the greatest amount of pressure concentration. The writer sees no theoretical justification for the obtuse-pointed design, and still less for a round-headed design, in either case.

To understand this matter thoroughly, it is necessary to go back to the reason for using ogival-headed projectiles at all. Why was a sharp point ever given to an A.P. projectile? The answer is, to concentrate compression stress and give it high intensity, in the hope that the plate would yield thereto before the projectile did. This hope was realised at the commencement when plates were soft and homogeneous. Then came the steel-faced compound plate which put up the standard of stress-intensity to more than the chilled cast-iron projectile could stand. The reply was the forged steel shell which brought victory once more to the side of the attack. The

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defence retorted with the all-steel plate with super-carburised and chilled face, raising stress-intensity again to a point at which the shell was the first to give way. Now the projectile gives battle with its nose swathed in a steel bandage to increase its power of bearing stress-intensity, and this expedient, for the time at least, has restored its supremacy.

It will be seen, then, that from the first the shell has been the one to make power of bearing stress-intensity the issue to be decided; and it certainly never assumed a cap with the intention of distributing pressure instead of concentrating it. A reversion to a flat head would have been the most effective way of doing that. Nevertheless, the cap *does* distribute pressure incidentally, and it is not impossible that, in view of its small size, it would not give adequate reinforcement, except at a much higher velocity, if it did not. The cap as a reducer of stress-intensity assists plate and shell alike, so the shell derives no differential benefit from it in that capacity; but it gives to the shell alone additional strength to bear compression stress, and that is where its advantage to the latter comes in.

If, in addition to laterally reinforcing the projectile's point, the cap itself were to disintegrate the hard face of the plate before the shell arrived, another reason would be furnished to explain the reduced liability to rupture of a capped shell. The writer, however, is inclined to think that a velocity high enough to save the soft cap from complete deformation would, *a fortiori*, be high enough to save the shell's point from pulverisation. It would, in fact, be not less than the third critical velocity (which, it is thought, must always greatly exceed bare perforating velocity), and then the cap would be unnecessary.

That the kinetic energy possessed by the cap when attached to the shell is not an essential is proved by the fact that a 2-in. W.I. plate placed in front of a hard face gives perfect protection from pulverisation on impact. This can only act as a lateral reinforcer, for of kinetic energy it has none. The writer's own punching experiments also prove clearly that high velocity *per se* is not an essential. The cap on a punch is quite efficient when velocity of impact does not exceed a few feet per second, provided its diameter is not too scanty. The W.I. plate quoted above is analogous to a cap with a diameter of several feet, and so should be independent of velocity as well as (as proved) of kinetic energy. Caps of usual form, however, have far too small a diameter to give adequate reinforcement by their resistance to *slow* stretching, and the writer believes it is for this reason alone that they fail to save the point of the shell until a certain velocity has been reached. If considerably more diameter were allowed to caps, their inefficiency at low striking velocities would probably diminish and possibly disappear.

But the fact that the success of the cap does not depend on it, is no justification for disregarding its kinetic energy. If this energy were very great, owing to extremely high velocity, there can be no doubt the flattening of the cap would not be complete before the occurrence of failure in the plate's face; in such a case the

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projectile on its arrival would find some of its work done for it. There are reasons, however, for believing that such velocity is not attained in usual practice. Chief among them is the fact that when an uncapped projectile is pulverised on impact—which, it is thought, implies higher stress-intensity than is involved in the flattening of any cap—a portion of the original face of the plate some inches in diameter is commonly found to have been driven back intact, showing that it has been able to bear all the stress-intensity of the earlier stages of impact, and has only yielded by its peripheral attachment to the plate giving way under the large amount of total pressure that is of later occurrence.[11]

The writer, therefore, prefers to hold for the present the view that the main function of the cap is lateral reinforcement of the weak point of the shell: that incidentally it reduces stress-intensity for both plate and projectile: and that, at current striking velocities, no serious preparatory work is done by it on the plate before the arrival of the shell itself.

Experiments might with great advantage be made to test against each other the two extreme forms of cap, namely, one with a flat head of the greatest admissible diameter projecting very slightly beyond the point of the shell, and the other with an ogival point, like that of the projectile, and projecting 2 in. or 3 in. in advance of it. If the former proved as good as the latter, it would be worth while to try the effect of making it of a stronger material than mild steel; for, although the long-pointed cap must be highly ductile, it does not follow that the flattened one need be more than moderately so.

All the foregoing remarks have reference to impacts normal to the face of the plate. The subject of oblique impact, though very important, is so intricate that considerations of space preclude more than a brief mention of it in these notes.

There are two kinds of oblique impact, of which only one—that of the inclination of the trajectory to the normal to the plate—has hitherto been generally recognised. In this kind the apex of the projectile is practically, if not absolutely, in the line drawn from its centre of gravity to the point of impact, and the blow of the shell itself is fairly delivered through its point. In the other kind, which involves still more serious risk of fracture to the shell, its own axis is inclined to its trajectory, while the latter may or may not be inclined to the normal to the plate.

Consider a 12-in. capped shell, loaded and fused, fired at vertical armour at 8,000 yards. Suppose also that on leaving the muzzle it has its axis pointing upwards 4°, its velocity of translation being 2,700 f.s., and of axial rotation 5,400 revolutions per minute. The gyrostatic action due to the axial rotation, though considerably reduced by air friction in flight, may be expected to remain powerful to the last; for, even if the speed of rotation were to lose as much in proportion as that of translation, which is

[11] The diagram in Fig. 14 (Plate III.), where striking velocity is about a practical maximum for the present day, seems to suggest a maximum intensity of stress on the plate of no more than 70 or 80 tons per square inch before the arrival of the shell itself.

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far from being the case, the former would be 3,300 revolutions per minute when the latter had come down to its remaining value for the given range of, say, about 1,650 f.s. The centre of gravity of the shell being no more than 0·87 in. in advance of the centre of figure, the "righting moment" of the air resistance has only a leverage varying from zero to a maximum of 0·151 in., and so must be very small (leaving out of account the additional complication of the precessional movement started when the position of a gyrostat's axis is forcibly altered); but, as its influence is at least in the desired direction, we may rely upon the air resistance not increasing the unfavourable angle, and so the worst case will be made out by ignoring it altogether, and assuming that the gyrostatic action is unopposed and retains the shell's axis pointing upwards at 4° when impact takes place and the trajectory is pointing downwards at 6°. Fig. 16 (Plate III.) illustrates this condition, and it will be realised that the angle (10°) between axis and trajectory is much more unfavourable for the projectile than a considerably larger angle would be between trajectory and normal to the plate. A "couple" with an arm about 4 in. long is formed by the momentum of the shell acting along its trajectory and through its centre of gravity, and the reaction of the plate acting in an opposite parallel direction through the point of impact. This couple must have a powerful cross-breaking effect on the shell.

If the roll of the attacked ship brings its side-armour normal to the trajectory at the moment of impact, it increases the obliquity between the axis of shell and the normal from 4° to 10°; and if the roll is in the other direction, and brings the shell's axis into the normal, it increases the obliquity of the trajectory from 6° to 10°. All this is supposing there is no obliquity whatever in a horizontal plane. On the whole, it would appear that truly normal impact at long range is not simply unlikely but may be actually impossible; whence it is a reasonable inference that the defensive power of armour under the probable conditions of a naval action will be greater than suggested by results obtained with the same striking velocity at short range on the proving grounds.

These notes have now reached their allotted limit, and the writer has to apologise to the Council for not having more literally complied with their request to furnish a paper on the "*Manufacture* and Resistance of Modern Armour." His excuse is that a description of the former was included in an able article by Lieutenant Jones, A.O.D., published in *The Engineer* less than twelve months ago. In conclusion, he desires to express his indebtedness to Mr. A. W. Dixon and Mr. W. C. Macartney, of Messrs. John Brown & Co., Ltd., and to Major Strange, late R.A., of Messrs. Thos. Firth & Sons, Ltd., for their kind assistance in carrying out experiments; and also to their respective firms for the provision of the necessary plant and materials.

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APPENDIX A.

Investigation of the Approximate Laws Governing the High-Speed Punching of Homogeneous Unbacked Plates.

(*See footnote* (1) *on page* 26.)

In the earliest days of the adoption of armour for Naval defence, the formula

$$t^2 = \frac{W V^2}{D} \times K$$

for the perforation of wrought iron was proposed by Sir W. Fairbairn, K being a numerical co-efficient. The statement of laws here involved is

$$t^2 \propto \frac{W V^2}{D}.$$

Now, if this statement of laws had been correct, a constant value for K would have resulted; but, on testing the formula by experience, even over the restricted range of velocities in use at the time, it soon became evident that, to make its results conform to those of practice, K required a different value for every substantial change of velocity. A list of suitable values was accordingly tabulated, and fairly satisfactory results were obtained over the somewhat narrow range of moderate velocities then in use; but attempts to adapt it to the higher velocities that a little later became practicable involved such large empirical changes in K that the formula was recognised to be unsatisfactory, and was abandoned.

That the formula would not work with an approximately constant co-efficient was, of course, conclusive evidence that its statement of laws was incorrect, in that the exponent of one or more of the factors was not what it should be, and later formulæ propose all sorts of empirical variations in the exponents of t, W, and D; but all, with one exception, retain V in the second power. The writer, so far as he is aware, stands alone in proposing a formula in which the sole variation from Fairbairn's is in the exponent of V; and that this alteration was not a purely empirical one will be seen from the following *résumé* of the investigation.

The act of perforating a W.I. plate with an ogival-pointed projectile is an act of punching, and, although the substitution of an ogival point for a flat head certainly introduces modifications of co-efficients, there seems no reason to suppose the difference extends to the laws of punching that govern the inter-relation of the various factors. It is, then, assumed that, as long as the laws only are under discussion and no change of form takes place in the projectile, the shape of its head is not material.

In punching a hole of a given size in a plate with a punching machine, experience has shown that we must provide force enough—that is, pressure enough between face of punch and face of plate—to maintain a given relation to the plate's thickness depending on the material. For punching boiler-plates, Molesworth gives the formula $F = 80\,t\,D$; the law thereby expressed being that the requisite force or pressure F between punch and plate must vary as the plate's thickness and the diameter of the punch; that is, $F \propto t\,D$.

It will be seen that when the plug or cylinder of metal opposed to the punch has been shifted a certain distance, failure by shearing takes place, and F at once drops to a small fraction of its original value, only friction remaining to be dealt with. We may, then, ignore the small residue of work[12] required still to be done to push out the sheared plug, and consider the total work of punching to be represented by the mean value of F multiplied by the distance the punch has moved after first contact with the plate till the severance of the metal round the plug is complete. This distance, taking into account the

[12] It is simpler to ignore this, but it is not essential. If we take it into account, we still arrive at the conclusion that the distance through which F has to be exerted is proportional to t.

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compressibility of the plug, may be expected to vary with the plate's thickness, and we may call it $m\,t$ where m is constant. The work done then varies as F $m\,t$, or as t^2 D.

If this work is to be balanced by the energy stored in a projectile, which varies as W V^2, we have—

$$t^2 \propto \frac{W\,V^2}{D},$$

which is the statement of laws expressed by Fairbairn's formula.

But we have seen that this statement of laws is admittedly and demonstrably at variance with facts. Must we, then, fall back on juggling in the dark, empirically, with the exponents of the factors, or is there reason to think that there is some flaw in the foregoing argument, the effect of which does not become apparent in cases of slow and deliberate punching, where V is almost negligible and approximately constant, but which leads us entirely astray when the argument is applied to high-speed punching where V is not only very great, but is liable to large variations?

The writer has formed the latter opinion, after investigating the assumptions made in the argument leading to Fairbairn's formula. These are—

(1) The work done, as determined by multiplying F, the mean pressure exerted, into the distance it acts through, must equal the kinetic energy of the punch due to its mass and velocity.

(2) F varies as t D.

(3) The distance through which F acts varies as t.

Of these assumptions the first is obvously justified, and careful experiments[13] made for the writer have completely verified the second for slow-speed punching; while the last, though not easy to verify, seems eminently reasonable. Therefore, all the assumptions may be accepted when the speed of punching is very low and not liable to much variation.

If we next examine the assumptions to see if any of them are likely to be falsified by wide alterations of speed, we find no apparent reason for suspicion, except as regards the second, which lays down that, when other conditions are constant, the mean pressure exerted by a projectile in perforating a plate varies as the plate's thickness. The writer thinks this is clearly incorrect, when velocity plays a substantial part in the production of F.

In catching a cricket ball with a rigid hand, the pressure is felt to be more than if the hand is allowed to yield. It is felt to be more with a swift ball than with a slow one, and less if the hand is allowed to yield much than if it is only allowed to yield a little. If the yielding is made independent of the will, except in so far as the will determines an initial setting or rigidity of the muscles, the hand will be pushed back to an extent proportional to some power of the velocity of the ball. That is to say, the greater the velocity the more the yielding. And we have seen that the more the yielding the less the pressure, and the more the velocity the more the pressure. Which, summed up, amounts to the statement that increased velocity has both an increasing and a decreasing effect upon the pressure.

It is obvious, then, that a proper expression for the variation of F must have the form $\frac{a}{b}$, where a and b both increase with the velocity, though not necessarily as the same power of it. For a put V^x

[13] These experiments, made with punches truly ground to nominal diameter and plates machined on both sides to exact nominal thickness, established the formula F = 75·11 t D. Adding 6½ per cent. to the co-efficient to allow for dull punches and rough plates, this fully confirms Molesworth's practical formula F = 80 t D. The late Captain Orde Browne, in his book, "Armour and its attack by Artillery," refers to experiments he made with ogival-pointed punches forming part of a falling weight, which also confirmed the assumption that the force necessary to punch the same plate varies as the punch's diameter.

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where x satisfies the statement $V^x \propto t$, and for b put V^y where y is unknown. The statement then stands $F \propto \frac{t}{V^y}$, which, it is submitted, is fully justified. The writer now makes the only assumption that can reasonably be called empirical in his whole argument by giving to y the value of unity,[14] and stating $F \propto \frac{t}{V}$, or, where D is not fixed, $F \propto \frac{t\,D}{V}$.

Accepting the other assumptions that led to Fairbairn's formula, we have—

F varies as	$\frac{t\,D}{V}$
Distance acted through varies as	t
Work done varies as F t, or as	$\frac{t^2\,D}{V}$
Time of action varies as	$\frac{t}{V}$
Power exerted varies as $\frac{t^2\,D}{V} \div \frac{t}{V}$, or as...	$t\,D$

From which we deduce—

Theorem 1.—*In the process of high-speed punching of a homogeneous plate, the average power exerted by the punch in obtaining perforation varies as the plate's thickness multiplied into the punch's diameter.*

This law for Power has been derived only from a consideration of the mean pressure on the punch and the distance through which it acts. The following is a statement and proof of a law for Power based on the mass and velocity of the projectile and the distance in which its kinetic energy is destroyed.

Theorem 2.—*If a body of weight* W, *moving with velocity* V, *is brought to rest by a retarding force in a distance* S, *the average power the said force must exert is proportional to* $\frac{W\,V^3}{S}$.

The work stored up in the body that has to be done by the retarding force varies as $W\,V^2$. The time during which the work has to be done is the time taken by the retarded body to pass through the distance S, so is directly proportional to S and inversely proportional to the mean velocity over the distance, which is proportional to V. Therefore the work is done in a time that varies as $\frac{S}{V}$. The power involved is directly proportional to the work done and inversely proportional to the time occupied in doing it, and so varies as $W\,V^2 \div \frac{S}{V}$, that is, as $\frac{W\,V^3}{S}$.

Combining the conclusions of Theorems 1 and 2, we have—

By Theorem 1.—Power projectile has to exert varies as $t\,D$.

By Theorem 2.—Power plate must exert to arrest projectile in distance S varies as $\frac{W\,V^3}{S}$.

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[14] This is not an off-hand guess, but results mathematically when $x = \frac{1}{2}$, which is a value agreeing well with experience and is more or less closely approximated to in all formulæ which obtain any reputation for reliability. The demonstration is as follows :—The mean effort that must be applied to a body of weight W to change its velocity from V to zero in a distance t is $\frac{W\,V^2}{2\,g\,t}$, so F, the mean pressure exerted on a given projectile by a plate in reducing its velocity from V to zero in a distance proportional to t must vary as $\frac{V^2}{t}$, and, substituting V^x for t, $F \propto \frac{V^2}{V^x}$. Multiply both numerator and denominator of this expression by V^{x-2}; then $F \propto \frac{V^x}{V^{2x-2}}$, so $y = 2x - 2$; and, if $x = \frac{1}{2}$, $y = 1$.

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Therefore—

$$(1) \qquad t \text{ varies as } \frac{W V^2}{D S}.$$

It will be seen that in a case of exact complete perforation (Fig. 12, Plate III.) $S = t + L$, and in Service projectiles L is so nearly three times D that 3 D may be substituted for L. We get then—

$$(2) \qquad t \text{ varies as } \frac{W V^2}{D (t + 3 D)}.$$

Now, when calibre of projectile and thickness of plate are equal, $D = t$, so that—

$$(3) \qquad t \text{ varies as } \frac{W V^2}{D \times 4 t}, \text{ that is, } t^2 \text{ varies as } \frac{W V^2}{D}.$$

Also, when calibre of projectile is three times or one-third the thickness of plate, $D = 3t$, or $D = \frac{1}{3} t$, and then—

$$(4) \qquad t \text{ varies as } \frac{W V^2}{D \times 10 t}, \text{ that is, } t^2 \text{ varies as } \frac{W V^2}{D}.$$

or

$$(5) \qquad t \text{ varies as } \frac{W V^2}{D \times 2 t}, \text{ that is, } t^2 \text{ varies as } \frac{W V^2}{D}.$$

From this it follows that, for every relation between calibre of projectile and thickness of plate, the law holds that—

$$t^2 \text{ varies as } \frac{W V^2}{D}.$$

This is the writer's formula of variation,[15] and that it is not without claims to constitute a substantially correct statement of the laws governing the perforation of armour may be judged from the following quite unsolicited letter received some time ago from a high official in one of the leading Admiralties of the world :—

> "My Government has lately been making experiments on the resistance of material to "perforation by projectiles. These experiments have been somewhat off the usual "lines as regards availability of data, and considerable use has been made of your "formula. You may be interested to know that the results reached have been almost "exactly in accordance with those calculated therefrom."

Disclosure of the name and nationality of the writer of this letter would, were it permitted, substantially add to the weight of the testimony.

So far we have only dealt with the formula of variation—

$$t^2 \propto \frac{W V^2}{D}$$

which is believed to be substantially applicable to the perforation of all classes of plates—hard or soft, homogeneous or face-hardened—and for all relations between calibre and thickness, when no work is done on the projectile ; but when we come to convert this into a formula of equation, and say—

$$t^2 = \frac{W V^2}{D} \times C,$$

the numerical co-efficient C must naturally be empirical, and must be dependent on the material being

[15] The formula could have been equally well arrived at by equating "Work" with "Work" instead of "Mean power" with "Mean power," but the method pursued, bringing in the length of the shell, has been preferred.

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punched, the shape of the punch's head, and the amount of work, if any, absorbed by the punch itself. Further, we see from (3), (4), and (5) that—

when $D = t$ ... ... $t^2 = \frac{W V^2}{D} \times \frac{C_1}{4}$,

„ $D = 3t$ ... ... $t^2 = \frac{W V^2}{D} \times \frac{C_2}{10}$,

„ $D = \frac{1}{3} t$ ... ... $t^2 = \frac{W V^2}{D} \times \frac{C_3}{2}$,

and we have no assurance that $\frac{C_1}{4}$, $\frac{C_2}{10}$, and $\frac{C_3}{2}$ are all equal. Consequently, the writer's equation formula for the perforation of wrought iron—

$$t^2 = \frac{W V^2 \log^{-1} \bar{9}{\cdot}1590}{D}$$

only strictly applies for the relation between calibre and thickness ($t = 3$ D), for which the co-efficient was chosen; and, to solve problems in the attack of wrought iron, that co-efficient might require to be varied a little when this relation alters. In practice, however, the formula is only required for solving problems in the attack of plates whose resistance to a given attack is equal to that of wrought iron N times as thick; and N, which is called the "Figure of Merit,"[16] may be so chosen as not only to represent the quality of the plate, but the relation between its thickness and the calibre of the attacking projectile, the shape of the projectile's head, and the liability or otherwise of the projectile to rupture, as well. The addition of a single numerical co-efficient to t, therefore, is all that is required to adapt the formula to any possible case.[17]

Of all the empirical formulæ proposed, the one most generally in use on the Continent is that of Commandant Jacob de Marre for the perforation of what is called "ordinary steel." It is—

$$t^{1\cdot4} = \frac{W V^2 \log^{-1} \bar{7}{\cdot}9813}{D^{1\cdot5}},$$

and the statement of laws is—

$$t^{1\cdot4} \propto \frac{W V^2}{D^{1\cdot5}}.$$

This may be written—

$$t^2 \propto \frac{W^{1\cdot4286} V^{2\cdot856}}{D^{2\cdot143}}.$$

A comparison of this with the Tresidder variation formula shows that when the left-hand side of each is t^2, the exponent of V is nearly the same in both. Those who have to frequently work ballistic formulæ, however, will not be slow to appreciate the convenience of a formula which has no fractional exponents. Not only is time saved, but the calculation is so simplified that liability to error in hasty working is very much reduced.

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[16] "Co-efficient of conditions of attack" or, as suggested by Major Wolley Dod, "Factor of perforation," is a somewhat better name for this co-efficient than "Figure of merit." The latter suggests (and was intended to suggest) the quality of the plate, but, as this is only demonstrated when exact perforation occurs, the expression in other cases may mislead. The co-efficient is the "Figure of merit," not that the plate necessarily has, but that it ought to have if it pretends to be a match for the round.

[17] For example:—For perforation of a 6 in. Krupp plate attacked by a 6 in. uncapped projectile (which will break) the additional co-efficient might be 2·7. For the same plate with the same projectile capped (which should not break) it might be 2. For a 12 in. Krupp plate with the same projectile capped and uncapped it might be 1·8 and 2·3 respectively.

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APPENDIX B.

Stress Diagrams of Projectiles in Collision with Plates.

(*See footnote* (1) *on page* 26.)

In view of the excessively brief instant of time occupied by the collision between a projectile and a plate, any attempt to construct a diagram showing graphically the variations of the stress to which the former is subjected may, at first sight, be thought hopeless. Nevertheless, it is interesting to ascertain how far one can get in the effort after the unattainable, especially as it may happen that investigation will show we really have more data to work upon than before realised.

Let us begin with the case of a 6-in. uncapped, but unbreakable, projectile of 100 lbs., 17·30 in. long, which exactly perforates a 12-in. homogeneous plate at 2,800 f.s.

First draw to any convenient scale a half longitudinal section of the shell (Fig. 12, Plate III), and erect five ordinates to the curve of its head so that their lengths shall represent respectively 1 in., 1·5 in, 1·75 in., 2·12 in., and 3 in. Number these 1, 2, 3, 4, and 5, the last being where the curve joins the parallel part of the body—that is, at 7·9 in. from the point. Not till the projectile has penetrated to this extent will its full sectional area be engaged, and as it penetrates to the other ordinates in succession the sectional areas engaged will be respectively $\frac{1}{9}$, $\frac{1}{4}$, $\frac{1}{3}$, and $\frac{1}{2}$ of the maximum.

Draw the base line $a\,e$ of the diagram to represent S to scale, so that it equals $t + L = 29{\cdot}3$ in., and mark on it the points b, c, and d, so that $a\,c = t = 12$ in. and $a\,b = c\,d =$ length of projectile's head $= 7{\cdot}9$ in. At e erect the perpendicular $e\,f$ to represent on any convenient scale the average[18] pressure $F = \frac{12\ W\ V^2}{64{\cdot}4 \times 2{,}240\ (t + L)} = 2{,}226$ tons. Complete the rectangle $a\,f$, whose area will then equal F S = total work done = total kinetic energy of shell $= \frac{W\ V^2}{64{\cdot}4 \times 2{,}240} = 5{,}435$ foot tons.

We know the stress diagram must enclose an area of exactly this amount. We also know the pressure must be zero at a, because this is the point of origin of the resistance, and that it must be zero at e, because the projectile is then at rest. So a and e are points in the curve. The probability is very great that maximum pressure will occur at, or about, the moment the point of the shell reaches b, because at any earlier moment the full cross-sectional area is not engaged, and at any later moment the waning velocity has nothing to compensate it. Assume for first trial that the maximum pressure $= 3\,F$, and erect the ordinate $b\,j$ accordingly. At c, the point of the shell having reached the original position of the back of the plate, pressure finds substantial and rapidly increasing relief, so the outline here must have a rapid downward inclination. When the point reaches d, the shoulder has reached the original position of the back of the plate, and little more than frictional work remains to be done, so that the ordinate of the outline at d will be a very small one, say, $d\,g$ as in Fig. 12 (Plate III.).

We must now consider the effect on the diagram of the shape of the projectile's head.

Fig. 13 shows the two curves of "engaged perimeter" and "engaged area" as penetration proceeds, the scale for the ordinates of the one having been so adjusted to that of the other as to make the maximum ordinates the same length for each, to facilitate comparison of character. It will be seen that the "engaged perimeter" curve is continuously convex during entry and continuously concave during exit, while the "engaged area" curve changes from concave to convex during entry, and from convex to concave during exit. As long as the action of penetration is a shearing action—which would be all the time with

[18] This is the average pressure over the distance of arrest (energy divided by distance), and must not be confused with the average pressure over the time of arrest (momentum divided by time), which is indeterminate if the time is unknown. The two are identical when the mean velocity during arrest is $\frac{V}{2}$, but not otherwise. See Note 19.

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a flat-headed projectile—the "engaged perimeter" curve is the one whose character should govern that of the pressure curve; but in the earliest stages of penetration of a pointed projectile, while the plate-metal is being squeezed out to the front, the resistance is, it is thought, for a short time rather proportional to area than perimeter, in which case the commencement of the pressure curve should be concave like that of the "engaged area" curve. Therefore it may be argued that the pressure curve should start at *a* concave to the base-line and soon change to convex, so continuing with increasing ordinates to *b*, and with decreasing ordinates to *c*; and that from *c* to *d* it should be concave.

A careful consideration of the changing conditions of resistance will thus enable a pressure curve to be drawn of substantially correct character.

We must now measure the area of the diagram obtained, and, if it exceeds or falls short of that of the rectangle *a f* the ordinate *b j* must be reduced or increased, and the whole outline gathered in or expanded to correspond, till the desired area is arrived at. The result is shown by the full line in Fig. 12 (Plate III.), the greatest ordinate of which, measuring 6,300 tons, indicates approximately the amount of maximum total stress. A diagram drawn in this way, with a good knowledge of the subject, will, when its area is a scale representation of F S, be a very fair approximation to a correct stress diagram under the given conditions.

The foregoing is a diagram of total longitudinal compression stress, but we can construct from it a stress-intensity diagram[19] as follows:—

After the shell's point has reached *b*, and until it begins to emerge from the back of the plate, the full sectional area is engaged; so from *b* to *c* the intensity curve will coincide with the total stress curve if we adopt for the ordinates of the former a scale that makes *b j* represent $\frac{\text{maximum total pressure}}{\text{maximum cross-sectional area}} = \frac{6{,}300}{28{\cdot}274} = 222{\cdot}8$ tons per square inch, in this case. Now erect the ordinates 1, 2, 3, and 4 at the same distance from *a* as from the point of the shell in the drawing we commenced with. Make No. 1 ordinate nine times the length it has for the total stress curve, and its end will then be a point in the intensity curve. Similarly, make Nos. 2, 3, and 4 ordinates respectively four times, three times, and twice their former lengths. The intensity curve can then be drawn through their ends and will be complete from *a* to *c*; see chain-dotted line in Fig. 12. The portion beyond *c* can also be drawn on similar principles, but is not of much interest. It will be seen that the maximum intensity of pressure measures about 400 tons per square inch.

The use of the stress-intensity curve is principally academical. It serves to illustrate the reason a projectile runs a diminished risk of being smashed up on a non-face-hardened plate—namely, that although it has to undergo a stress-intensity quite sufficient to break it, even in the very brief instant available, if it had only its own strength to rely on, that stress-intensity does not occur till the head is embedded to some distance in the plate and is thereby virtually "capped."

The pressures we have been considering are only the components parallel to the axis of the shell of all the pressures its head encounters after becoming embedded. There are other components perpendicular to its axis, but these are friendly; they reinforce the shell against lateral rupture and at the same time counteract each other.

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[19] A velocity curve whose ordinates will represent velocity at any instant when the corresponding abscissæ represent penetration (not lapse of time since impact) can also be drawn through the ends of ordinates whose lengths are $\sqrt{\frac{W V^2 - 2ga}{W}}$, where *a* represents the area of the pressure diagram up to the ordinate concerned. This curve is shown plain-dotted in Fig. 12, but the mean length (1,080 f.s.) of its ordinates does not represent the time-average of velocity. The curve tells us approximately, however, the mean velocity at which every tenth of a foot (say) was negotiated, and, therefore, the time taken in traversing each tenth of a foot; the sum of all which times is $\frac{1}{10}$ sec., and is the total time of arrest. Of this, less than one-tenth elapses before shoulder of shell is level with back of plate. The time-average of velocity comes out at 122 f.s., and the time-average of pressure at 194·1 tons, the last one-third of the distance taking more than nine-tenths of the time. (See Note 18.)

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The shell is also subject to torsional stress due to the head being "braked" by the plate while the inertia of the body strives to maintain the axial rotation imparted by the rifling.

The total stress curve has a practical use—or, at least, the determination of maximum total stress has—in giving an idea of the statical pressure a test-plate target, or an armoured structure, must be designed to resist.

Next let us consider the alteration that would be necessitated in the diagram if, in the case just dealt with, the plate had been hard-faced instead of homogeneous, the shell being still considered unbreakable though not capped (Fig. 14, Plate III.).

The mean pressure and the area the diagram must enclose remain unaltered, but the maximum pressure occurs earlier and its amount is probably less. The reason it is earlier is obvious; the reason its amount may be less is partly because it occurs before the maximum area of plate-metal is engaged, and partly because the resistance to displacement of the molecules of a crystalline structure collapses suddenly as soon as any actual displacement of them takes place, and this is facilitated by the higher intensity of stress due to its concentration on a smaller area. It is this setting forward of the moment of maximum pressure, with the view of subjecting the point of the shell to an insupportable stress-intensity before it is embedded and so reinforced, that is the great object of the hard face. A maximum stress-intensity of something like 1,300 tons per square inch is suggested by this diagram.

Next, suppose a Firth cap, projecting 1·2 in., to be added to the shell, but without increasing its weight, and let the overall length be 17·71 in. This modifies S to 29·71 in., and F to 2,195·3 tons, or 30·7 tons less than before. Work done—that is, area of diagram—is not altered.

A circumstance immediately claiming notice is that for 1·2 in. after impact the plate has nothing to do but to flatten the cap, and although it has to do this very rapidly it is assumed that its own hard face is not broken into in the process. The pressure curve now begins 1·2 in. before the face of the plate is reached by the projectile, which suggests the alteration shown by the dotted line in Fig. 14. This is about the only alteration required by the total pressure curve, beyond the trifling modification necessary to keep the area equal to F S, as the cap bursts, and its influence disappears quite early; but the stress-intensity curve is very much modified, for when the point of the shell first gets contact with the plate the area engaged is that of the crushed head of the cap, probably 7 to 12 sq. in. instead of a minute fraction of a square inch. Of course, this reduces stress-intensity for the plate as well as for the projectile, but the reduction is accompanied for the latter by reinforcement of strength and for the former it is not. If the cap adds 9 sq. in. of area to the cross section at No. 1 ordinate (which is probably about the limit of its influence) it multiplies it by about four. Therefore the intensity ordinate here would be reduced from nine times to two and a quarter times the total stress ordinate, and the maximum stress intensity from 1,300 to 325 tons per square inch, while at the same time the lateral strength of the shell is highly reinforced.

Lastly, take the conditions of the previous case, but without a cap; weight still 100 lbs. The shell cannot now escape pulverisation on impact which will prevent it from attaining perforation. Assume that it gets a penetration of 6 in. The energy to be absorbed will be 5,435 foot tons, the same as before, and, as S is only 6 in., F is increased to 10,870 tons (Fig. 15). The question now is at what point will maximum pressure occur, and this question we cannot answer with any confidence. We know the point must fail before it has obtained more than a very slight penetration, and that its failure involves the almost simultaneous destruction of the whole shell. We know also that the pressure curve must nowhere have a tangent parallel to the face of the plate, as that would mean a pressure increment without a penetration increment, which, as drawn attention to at the outset of these notes by the illustration of the arrested snowflake, is impossible. Therefore the curve must leave the base-line at *a* and rejoin it at *e* at an angle that is acute towards the other end of the line. Any acute-angled triangle on the base *a e* with a perpen-

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dicular height of 2 *e f* would be a possible solution; but a much more probable one would be a continuous curve, as indicated in Fig. 15 (Plate III.) enclosing an area equal to that of the rectangle *a f*.

It will be observed that maximum total pressure in this last case, which can hardly be less than 14,000 tons, is more than double that in either of the cases where perforation was obtained. The supporting structure of the plate will consequently be subjected to additional stress, which would require notice in its design.

Stress-intensity here becomes something prodigious, which is not surprising, seeing how small a distance is allowed for stopping this high-velocity projectile. Further reduction of S would increase stress intensity at an appalling rate, for the limit, when S becomes zero, is not simply an infinite number of tons per square inch, but an infinite number of tons on a zero number of square inches.

Whatever outline is thought to best represent the variations of pressure in any particular case, the diagram must have S for a base, must enclose an area equal to F S, and must have an outline which is at no part parallel to the face of the plate. A consideration of these diagrams, if of no other value, will assist realisation of the enormous advantage a hard face gives to a plate if it smashes the shell, and its uselessness,[20] or worse, if it does not.

The principles on which approximate stress diagrams can be constructed apply equally to cases where the projectile is not arrested, but proceeds onwards unbroken after perforating the plate. In such cases, however, it is necessary to know the velocity of exit as well as the striking velocity, and two conditions of stress previously negligible become of importance. One is that longitudinal compression suddenly changes to tension as the head emerges, owing to the inertia of the part of the head first freed wanting to continue motion at a faster rate than the still retarded body can keep up with; the other is that torsion of entry due to the head being "braked" while the body tries to maintain speed of axial rotation is succeeded by torsion of exit in the reverse direction, when the body is being "braked" and the head is striving to set the pace of the "spin." A clean cross-break in some part of the shell is a very frequent result.

The "Power" developed by a plate in resisting a projectile, though, of course, exerted for a very brief time, has an amount perhaps hardly generally realised, and may be worth drawing attention to. At Shoeburyness it is not unusual for a 12 in. plate to show only 2 in. or 3 in. penetration after the impact at 1,850 f.s. of a 12 in shell of 720 lbs. Allowing for some of the penetration being only temporary, and for the spring of the target, let us assume the shell is not really arrested in less than 6 in. If the reduction of velocity were uniform over the period of arrest—and there is no reason in this case for expecting much error in such an assumption—the average velocity during the period would be $\frac{V}{2}$ but, to avoid risk of exaggeration, let us take it to be only $\frac{V}{3}$. Then the time of arrest is $\frac{3 \times \cdot 5}{1,850} = \frac{1}{1,233}$rd of a second, or $\frac{1}{73,980}$th of a minute.

$$\text{Work done} = \frac{W V^2}{2g} = \frac{720 \times 1,850^2}{64{\cdot}4} \text{ foot lbs.}$$

$$\text{Average power} = \frac{720 \times 1,850^2 \times 73,980}{64{\cdot}4 \times 33,000} = \text{over } 85\tfrac{1}{4} \text{ million horse-power.}$$

[20] M. Bertin, in his excellent paper read before the Institution in 1897, concluded from his investigations that "the resistance of Harveyed plates is exactly the same as that of those which are not Harveyed when the projectile is not ruptured."

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APPENDIX C.

Thickness of Hard Face.

As we have divided the nature of armour resistance into three classes, (*a*), (*b*), and (*c*), of which (*a*) has the defect of resisting too suddenly, (*b*) of not resisting suddenly enough, and (*c*) is betwixt and between, it becomes important to decide to what extent (*c*) should partake of the (*a*) nature and how much of it should be (*b*). In other words, what is the best proportion between the crystalline part of a (*c*) plate and its whole thickness. Experience has shown that quite a small thickness of hard layer suffices to break the point of a projectile, *if it is well held up*, and if there is no soft layer in front of it. Increase of the proportion of crystalline structure reduces S largely, but only increases F very slightly, if at all. Therefore, more hard face means less F S, and involves a less resisting plate, and one that does not hold up the hard layer as well; and, as the initial pressure cannot exceed the "holding up" force,[21] this defect may result in insufficient initial pressure to pulverise the point of the projectile. The plate then finds itself opposed to what is for all practical purposes a capped shell, and will be perforated if it is being tested with velocity suited for an uncapped one.

A most striking experimental illustration of this is available. Exactly similar conditions of firing test were applied to three plates which, as far as care in manufacture could secure uniformity, and elaborate tests could demonstrate it, were identical with each other in all respects save that of the proportional depth of face-hardening.[22] In one, the crystalline part of the fracture was 63 per cent. of the whole, and this one was repeatedly perforated by uncapped projectiles as if they had been capped; that is, the points were not pulverised on impact, and the perforations were more or less clean cylindrical holes. In another, 55 per cent. of the fracture was crystalline. One projectile, out of several fired, perforated this, though not as easily as before, as it broke, and part fell in front of the plate. In the remaining plate the crystalline portion of the fracture was 29 per cent., and this smashed up all the projectiles completely.

These experiments indicate clearly that 29 per cent. of face-hardening is better than a larger proportion. It is quite likely that as little as 10 per cent. would give good results; 20 per cent. certainly does.

The success of the uncapped shell at the first of these plates was not on account of any approach to the third critical velocity, but was due to an abnormally high value of the first one. The initial pressure was insufficient, not for want of proper hardness in the face, but for want of adequate holding up.

APPENDIX D.

Backing.

It might seem at first sight that the addition of backing to a plate would always add something, if only a little, to its resisting powers; but this is not necessarily the case. Backing only helps the F factor; it hampers the S. If the F factor plays such an important part—as in the case of a hard-faced plate against an uncapped shell—that perforation can only occur by the smashing out of a huge conical disc, a yard or more in diameter, from the back, the presence of backing through which this great disc must be thrust broadside on to effect perforation of the target is obviously of enormous assistance. But if the shell, by being capped or otherwise, retains its form, and only tends to punch a clean hole of about its own diameter, backing adds to its task but an insignificant fraction. In these cases backing is helpful, more or less. But there is another case that is likely to have increasing prevalence since the introduction

[21] Any more than the pressure on a punch can exceed the pressure with which the plate is held up to it.

[22] So called. The really hard layer which will break a punch seldom exceeds about ¼ in. in thickness; but of course the crystalline portion usually called hard-face may, in thick plates, be several inches.

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of caps, and that frequently occurs when low-velocity projectiles of great weight are matched against comparatively thin plates. This is when the plate—its hard face discarded as useless under the given conditions of attack—is designed to give battle more with its S element than its F. Then the presence of backing in contact with the plate is wholly detrimental. It does not add to its resisting powers, but only hampers its movement and holds it up to punishment.[23] For this reason, the decision as to backing or no backing must obviously depend (*apart from considerations other than those of ballistic resistance*) upon the nature of the attack to be provided against and the way it is proposed to meet it. If the most is to be made of the S factor of any plate, it is not necessary to do away with the backing, but only to erect the plate out of contact with it—say, by putting 2 in. sleeves on the bolts between backing and plate. Then, when a heavy shell strikes at low velocity, all the energy required to bend the plate to touch the backing will be lost to the shell before its real work of perforation begins.

APPENDIX E.

Punching Experiments.

Punch.	Plate.	Net Pressure per sq. in. on Ram of 125·185 sq. in., in lbs.	Actual Punching Pressure in lbs.	Pressure in lbs. required by the Formula $F = 75{\cdot}111\ t\ D$ tons.
½″	⅛″	84	10,515·54	10,515·54
	¼″	168	21,031·08	21,031·08
	⅜″	252	31,546·62	31,546·62
	½″	336	42,062·16	42,062·16
1″	⅛″	168	21,031·08	21,031·08
	¼″	336	42,062·16	42,062·16
	⅜″	476	59,588·04	63,093·24
	½″	658	82,371·61	84,124·32
2″	⅛″	364	45,567·30	42,062·16
	¼″	672	84,124·32	84,124·32
	⅜″	1,008	126,186·48	126,186·48
	½″	1,344	168,248·64	168,248·64

The above results confirm the statement that, when the diameter of the punch is fixed, the punching pressure varies as the plate's thickness, and that, when the plate's thickness is fixed, the punching pressure varies as the punch's diameter.

[23] If a wood fence were put at the back of a cricket-net the netting would suffer more than if the fence were absent; even, it is thought, to the apparently paradoxical extent that both net and fence might be perforated by a ball that the net alone would stop. It would be like catching a swift cricket-ball while the back of the hand was pressed against a wall—an experiment not to be recommended.

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DISCUSSION.

Mr. C. E. Ellis (Associate Member of Council): I feel that I should be wanting in respect to my friend and colleague, Captain Tresidder, and also wanting in appreciation of the very able paper he has written, if I did not make a few remarks upon it. The paper, as all of you have seen, is of a very highly technical character. Captain Tresidder's mind is of an essentially logical turn, and, scrutinise this paper as you will, I, for one, am unable to find a flaw in any of the reasoning which he has advanced. There is, of course, one very serious difficulty in any attempt to make a paper on armour plates of what you may call a popular character, and that is, that the Governments of the world are only human, and they do not see why they should give the results of armour plate trials, carried out for their own benefit, to the rest of the world. Therefore, it is not possible for Captain Tresidder, or anyone else, to tell the world, through this Institution, what results have or have not been obtained either by armour plates or by projectiles in the various attacks to which the plates have been subjected, and that is one of the difficulties which the author had when he prepared this paper, but I am sure you will all agree that it is one of intense interest, and that the thanks of the Institution are due to him for the trouble and care he has bestowed upon it. There are one or two points I should like to call attention to in particular. First of all, the paper shows that the old fight which has been going on between projectiles and armour, I think now for forty-four years, still goes merrily on. It may be that the capped projectile is, at the present moment, the winner, but I daresay that is a position of honour which it will not hold, at any rate unchallenged, very much longer. The paper shows very clearly, I think, the nature of the best plate which can be made to meet the two forms of attack—by uncapped and by capped projectiles; and I venture to think that, under any circumstances, the best plate for the Governments of the world is the plate which is being made to-day. It may be that we can make a plate more suitable for meeting the attack of a capped projectile only, but a ship has to fight under any and all circumstances, and, so far as I am aware, there has been no trial of any plate where such generally good results have been obtained—results that would meet any form of attack from any kind of projectile—as with the present Krupp plate, which is now universally adopted.

Captain Tresidder points out one or two essential conditions for the best plate for resisting attack, and I do not think that the points which he makes can be too strongly emphasised. One is, do not make the hard face too deep; another is, see that you get toughness in the back of the plate. If you do that, you have done all that is reasonable, and you can go to your trial and come away, if not with flying colours, at any rate, feeling you have done all that could be expected of you. The curious effect of the attack of a projectile on a Krupp plate is that, although the impact is, perhaps, very much localised, the stress of the attack is spread over such a very large area at the back of the plate so that the resistance to penetration is multiplied to a very large extent. In the old days the whole thing was localised, and there was nothing to help that part of the plate which was attacked, but now, thanks to the toughened back in Krupp armour, we have more or less solved that difficulty. I do not propose to touch upon the question of the form of cap, as Captain Tresidder has referred to it in his paper with some minuteness, and we have heard the results of some interesting experiments he made with actual caps under pressure; but if any of you take the trouble to read the appendices to this paper, I think you will agree with me that they contain most interesting and useful information which, had it not been for the exigencies of space, might well have been embodied in the paper itself. There are points connected with backing which I consider of very great value. Captain Tresidder also points out very clearly, from experiments which he has himself seen, the value of limiting the proportion of the hard face of a Krupp armour plate.

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Mr. W. E. Smith, C.B. (Vice-President): My Lord and Gentlemen, as Mr. Ellis, who was kind enough to read Captain Tresidder's paper, has stated, this subject bristles with extreme difficulties; first of all, those imposed by the necessity of official secrecy on certain important points; but there are other real difficulties besides those. When we bear in mind that nearly all our experiments on the resisting power of armour plates have to be made on armour plates each of which costs, say, about £2,000, and on which you can only get three shots, the cost of the armour plate, apart from the cost of the shot and the use of the gun, and everything of that sort, means something like £700 for one experiment. Under those circumstances, of course, we are not at all in the same position as a chemist in his laboratory, who can make valuable experiments with materials costing only a few pence, the value of his experiments depending upon his procedure and his skill in utilising his material, whereas we suffer from the heavy cost involved. It is, therefore, a matter of very great satisfaction to find a gentleman of logical turn of mind like the author, devoting himself with such energy to carefully considering all the circumstances surrounding the behaviour of armour plate under trial, and endeavouring to modify both the nature and the velocities of projectiles, so as to bring about an approximately correct result, where no actual data are available. As a member of the Council, and as the one who suggested Captain Tresidder's name as a possible writer of this exceedingly good paper, I desire to tender him our best thanks for the trouble he has taken in its preparation, and the excellent way in which he has done it, and to inform him that this paper will constitute a very valuable addition to our Transactions.

As an Admiralty Officer assisting the Controller in armour plate matters, I am concerned with various circumstances in connection with the acceptance of armour from armour plate manufacturers, from the point of view of whether the material does or does not satisfy the conditions of contract, and there, again, we have very great difficulties. These difficulties are sometimes of a scientific and engineering nature, and sometimes they are inherent in human nature itself. In the case where the armour plate manifestly outmatches the conditions of the test imposed upon it, the manufacturer and the Admiralty officer are both satisfied. But sometimes the armour plate does not behave in that way, and the projectile goes straight through it; then the manufacturer wishes there were some method of absolute, as distinguished from a relative, system of testing armour plate, which would not be subject to all the vagaries of projectiles. That is a point of view to which, as engineers, we must all be continually giving consideration, so as to devise tests of armour plate that will not involve any considerations of the quality of the projectiles with which they are tested. There have been the same difficulties in other branches of engineering, but in these cases they are being more or less overcome. Magnetic tests, for instance, used to have reference to the magnetic properties of a particular magnet, but now absolute methods are in use about which there can be no question or dispute. We have all thought a great deal about it, but up to the present time with little or no satisfactory result. As an instance of the great amount of trouble and thought that Captain Tresidder has bestowed upon this paper, I may mention that in order to verify the old story about the candle and the door, he improvised a gun and with a candle he tried the experiment, using different velocities, with the results mentioned in the paper. In addition to the general abstract of the paper read by Mr. Ellis there are the appendices, and these are full of closely reasoned details. I have known Captain Tresidder for many years as an earnest student of armour plate matters, and I have derived much advantage from this association. The subject is an extremely difficult one, but Captain Tresidder's methods are undoubtedly sound as regards the broad results reached within the limits necessary to use them in ordinary cases. As our knowledge increases modifications may have to be made, but I am sure that the logical scientific analysis of Captain Tresidder cannot fail to assist us in the careful study of this important and intricate problem.

Mr. R. A. Hadfield (Associate): My Lord and Gentlemen, I should like to say how much indebted I feel to Captain Tresidder for the very interesting contribution he has given us on this important subject. I am sure we much regret to know the cause of his absence, and would offer him our deepest sympathy. Looking over the paper, I should like to make a few comments on pages 27 and 28. In the second paragraph on page 27 the author refers to the question of plates having toughness throughout. By

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that I presume he means a plate which is not cemented or hard-faced. Of course one can have a cemented plate which is hard and stiff on the face, even if it is not hardened. Perhaps it may some day be possible to manufacture a plate with a self-hardened face, which may be even better than one having a face of considerably greater hardness, but which is necessarily more brittle. We are greatly indebted to Captain Tresidder for his formula regarding the qualities of plates. This formula, as you know, is based upon the resistance of wrought iron, and, although that seems a simple fact to arrive at, yet it has been of the greatest possible value to those who have to deal with experiments on armour in different parts of the world. By means of Captain Tresidder's formula we can compare one result with another. In his formula the F.M.—*i.e.*, "figure of merit"—of a wrought-iron plate is taken as unity. As Captain Tresidder has referred to this question of tough armour, naturally one first of all takes into consideration what is the toughness of wrought iron. We all know that wrought iron is one of the toughest materials made even to-day. But wrought iron has this disadvantage—that it has not sufficient resistance—it is too plastic. Next comes compound armour, which has about 1·2 or 1·3 figure of merit, and then Harveyed plates, which have a still higher figure of merit. Beyond that we come to the K.C.—or Krupp cemented—armour, which varies in its figure of merit from 2·2 up to as much as 3·00. The two latter types are hard-faced material, but I cannot help thinking a time is coming when the disadvantages of hard-faced material will be recognised, and that we may come back to material having high tenacity, but, at the same time, great toughness. My firm has worked for some time in trying to introduce a material of this kind—that is, one which is not plastic like wrought iron nor a hard face like K.C. armour, but with the figure of merit still very fairly high, about 1·6 to 1·8.* I think, if it is possible to obtain armour of that character there is a very great future for it. I do not for a moment intend to say that we shall abandon the use of K.C. armour, but for certain kinds of work I think there is a great opening for material which can be cast into shape and which has also a fairly high figure of merit, such as I have mentioned, of about 1·6—that is, under the attack of capped projectiles.

In speaking of armour, one must not forget the great change brought about by the use of capped projectiles, for it is hardly likely that a battleship would to-day go into action with uncapped projectiles, and this fact greatly reduces the figure of merit of K.C. armour. As an instance of this, I may mention that in one particular case, my own firm had a trial of their capped shot abroad, and we got through K.C. armour at about 1·68 F.M. There are also other firms in this country—Messrs. Vickers and others—who are, no doubt, able to accomplish the same result. I am sure we are all much indebted to Captain Tresidder for pointing out the important quality possessed by tough armour, which I can thoroughly confirm from the results obtained in large numbers of experiments carried out by my own firm, and he has given us an excellent reason for believing that there is a future for material of this class. He sums up the position at the top of page 28: "So far, on the assumption that the projectile is unbreakable, the balance of advantages seem to lie with the soft plate, apart from the fact that it is not liable to crack, which the all-hard plate is." Some makers of K.C. armour would probably say that certain kinds of armour, even K.C., is not liable to crack; but with hard-faced armour the fact remains that its face is hard, and, therefore, its toughness must be less than that of material which has not a hard face. On page 31 Captain Tresidder advances a theory with which I do not quite agree. He says: "Below the first and above the third critical velocity the projectile needs no cap, because it is strong enough without one." As far as our experiments have gone at our own proof butt, this is hardly confirmed; in other words, if you fire an uncapped shot—say of 6-in. calibre—at a 6-in. K.C. plate at such a velocity as that it would go through, and then reduce that velocity, when you once get below the perforation point, I think that shot will break. I rather understand Captain Tresidder to mean that it will not do so.

Mr. ELLIS: The statement is based on actual experiments.

* This is not far short of K.C. armour, which, when attacked with capped projectiles, has its figure of merit brought down to about the same value.

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Mr. HADFIELD: We have tried experiments, and find that uncapped shots will break.* Then, with regard to footnote 6 on page 29, I think we ought also to give credit to Mr. Johnson, of America, who worked very hard at the problem of introducing capped shot; whilst, no doubt, Captain English was the originator, yet Mr. Johnson, of America, who was largely backed up in this country by Messrs. Vickers, did a great deal of the pioneer work in introducing capped shot. On page 30, Captain Tresidder refers to a double-ended cone being formed. I have followed a large number of results at Shoeburyness, and we do not always find that cone occur. I admit I have seen it sometimes, but it does not necessarily always occur.

As regards experiments with pressed caps, I carried out similar experiments three or four years ago; in fact, I exhibited at the Institution of Civil Engineers a cap to which we had applied pressure in the way now described by Captain Tresidder. (Producing specimens.) We got a little further than any of the caps shown here, and absolutely made the hole. Speaking of the results obtained with capped projectiles, I may refer to a very curious result here which members may like to see. (Specimen produced.) This was a 10½ cm. (4⅛ in.) shot weighing about 32 lbs. fired from a Krupp 10½ cm. gun at our own experimental butt. The cap I used was an ordinary armour-piercing cap. We did not attack a hard-faced, but a soft-faced plate; we were anxious to see what actually occurred, and thought we should have a better opportunity of catching the pieces of the cap than if we fired against hard-faced armour, and that was the result. (Indicating.) The energy acts so instantaneously that you can quite understand this cap losing its shape, but you would hardly expect it to form this peculiar ring. You can almost determine the rifling of the gun from the pieces recovered. I suppose caps act more or less in the same manner on hard-faced plates. I also have a piece here from a cap that was only fired at Shoeburyness about a fortnight ago. (Specimen produced.) That is a piece of a cap weighing 30 lbs., so you can imagine what enormous energy is brought to bear instantaneously. You can see how the material suffers. We obtained a very curious result, and one of special interest. We were attacking hard-faced armour, and the cell was covered over at the top, by a rope mantlet, which was suspended. The shell—a capped shell against a K.C. plate—struck the plate, and the fragment of cap now exhibited was found on the top of the butt embedded in the mantlet. I was also present at a similar experiment a short time afterwards, and when I again went on the top of the butt, the mantlet was there in exactly the same position as on the previous occasion, and a similar piece of cap was embedded again. The remarkable point is that when the projectile strikes the face of the plate one would almost expect that the pieces from the hard layer would rebound at the same time as the cap, but it was not so. The pieces of the cap rebounded, and they had time to get embedded in this mantlet, and the mantlet had time to turn over, and all this occurred, comparatively speaking, long before any of the pieces of the plate were thrown back. Under the shelter butt, which was about 100 yards away, we knew that pieces of the plate came back, because we could hear them going over the top of the butt. Many of these results, it will be seen, are largely dependent upon the question of time, though we can hardly talk of this in the ordinary way, because they are such infinitesimal portions of time from our point of view.

Captain Tresidder refers to certain punching experiments. Some years ago we carried out some experiments at Krupps' proving ground, where we fired a 6-in. flat-headed armour-piercing shot at a 6-in. K.C. plate, with no cap and no special point. It behaved in exactly the same way as a large punch. Here is the large piece punched out. (Specimen produced.) It is remarkable as showing that even if you do not have the ordinary ogival-pointed shape, the projectile is nothing more nor less than a punch. In some of the more recently made armour plates the quality, as Mr. Ellis rightly says, has been greatly improved, sometimes, as we know, at the cost of the projectile maker, because we are liable to rejections from that cause. Still, on the whole, it is much better for us to have higher quality plates if of uniform nature, and to know exactly what we have to face. In some of these more recently made plates one notices

* I have now definite evidence from an actual trial that a capped shot fired normally at a K.C. plate one calibre in thickness, striking with a velocity of about 1,000 f.s., hardly makes a mark on the face of the plate, the shot at the same time breaking into a large number of pieces.

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the peculiar action referred to by Captain Tresidder—that is, the production of very large discs. I do not know whether it will be quite believed, but I assure you that we have had, in some cases, discs of no less than 5 ft. diameter produced by the impact of a 12-in. projectile, and those results were obtained from plates of excellent quality. It shows the great energy which has to be resisted on the impact of one of these heavy shells. I also think it proves the wisdom of this country adopting the heavy gun and the heavy projectile.

Captain R. TUPPER, R.N. (Visitor): My Lord and Gentlemen, one of the remarks made in the paper by Captain Tresidder is that the projectile strikes the plate at long range with the direction of its axis at the same angle to the horizon that it had when it left the gun. From practical experience, having seen several shots fired at long range, and the result of them, it appears to me that this theory is correct. The gyrostatic action keeps the projectile at the same angle that it had when it left the bore, and so long as the gyrostatic action is sufficient to keep it straight, and the spin is sufficient to keep the point up, the projectile will not strike point down, as one would imagine it might, from the curve of the trajectory. I cannot say that this is absolutely correct in all cases, but I have noticed it in certain cases, and believe that it is so in most cases.

Mr. E. F. COAST (R.C.N.C.): My Lord Glasgow and Gentlemen, there are one or two points in connection with the appendix to Captain Tresidder's paper which have interested me very much, and I should like to ask one or two questions, in the hope that the author's reply may benefit others besides myself. If you refer to Appendix A you will see the author gives as the law for the perforation of homogeneous plates $t \propto \frac{W}{D} \frac{V^3}{S}$. In previous investigations of the formula I have generally seen S expressed directly as $\propto t$, as the author has done in explanation of Fairbairn's formula, but in Appendix A Captain Tresidder has now adopted a different argument. It would appear from the author's statement at the end of paragraph 5 in Appendix B that most of the work is done by the time the shoulder of the shot reaches the back surface of the plate, that S is more likely to equal $t + h$, where h is height of point of shot. As h has a constant ratio to D in shot whose heads are struck with radius = 2 D, we get formula (2) in the form $t \propto \frac{W V^3}{D (t + 1{\cdot}32 D)}$ for shot of all calibres. In passing from formulæ (2) to (3), (4), and (5), given on page 39, the author has put in a relation between t and D, and yet he obtains an expression still containing both t and D. Why cannot all the D's be expressed in terms of t, since there is a relation between them? If we do this, we get $t^3 \propto W V^3$.

On page 40 the author explains the use of the figure of merit N. Now, N is dependent on $\frac{t}{D}$, and when $\frac{t}{D}$ changes we should expect N to change, and actually N does then change. This was well illustrated by Captain Tresidder in some examples given in Brassey's Annual of 1905, and is also shown in footnote 17. Moreover, when $\frac{t}{D}$ is constant, hard-faced steel armour plates made by, say, the K.C. process give different N's. This can be seen by a table given by the author in Brassey's Annual of 1905 for uncapped A.P. shot. So that in order to know the limit of velocity for any plate to any attack, we must know values of N for all sorts of thicknesses of plate attacked by all sorts of diameters of projectiles; so we do not seem to be much nearer getting a constant co-efficient than Fairbairn was. When we come to the problem of finding the limit of velocity for, say, a 7-in. plate being tested by 9·2 A.P. shot, whereas the limit of resistance of the plate taken as the basis (which is, say, a 6-in. plate) is only known to 6-in. shot, Captain Tresidder's formula does not help us much, since we do not know N. Even if we try one plate against the attack of two different projectiles, we get different N's for the two attacks. Of course, we can make a near guess at N from any experience we may have had, but there ought to be some more systematic method than this. In this connection, it would be interesting to know what was the unusual nature of the experiments carried out by the foreign Admiralty, referred to in the paper.

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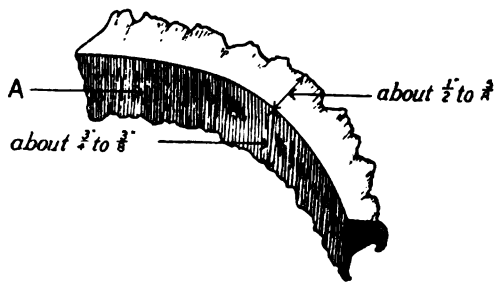
The co-efficient N is very much like our old friend the Admiralty co-efficient for E.H.P. It may be a convenient method of expressing the performance of a plate against a given attack, but whether or not this simple formula represents the actual law of perforation is another matter. Naval architects, when they found the Admiralty co-efficient method was not applicable to considerable changes of form and speed of ships, investigated further, and we now know that the E.H.P. of a vessel of given form can be calculated from two terms, one of which depends on the form and contains a constant co-efficient peculiar to the form, and the other term depends on the absolute size of the vessel. Now, it may be that the actual law of perforation of homogeneous plates depends on two terms, one of which depends on the ratio $\frac{t}{D}$ and is the same for all similar plates when $\frac{t}{D}$ is the same, and the other depends on the actual value of t. Captain Tresidder says he believes his formula is applicable to the perforation of all classes of plates, hard or soft, homogeneous or hard-faced. In this connection it is interesting to note that Krupps' formula for the perforation of hard-faced steel armour, which I believe is on the basis of experiments made by Krupp, is $t^2 = \frac{W}{D}\left(\frac{V}{150}\right)^3$, the law of which is the same as in Fairbairn's formula for wrought iron—viz., $t^2 \propto \frac{W V^2}{D}$. How far that dynamical law is verified by actual experience with hard-faced steel plates is, of course, only known by those who may be in possession of suitable data.

Mr. J. L. Samuda Benthall (Member): Lord Glasgow and Gentlemen, I only want to confirm one or two things Mr. Hadfield has said about caps. I have seen a good many shell and shot fired during the last five years, and have invariably noticed that the cap, if it is made the right size, will expand into a sort of plate. Roughly speaking, the cap of a 12-in. shell will expand into a plate about ¾ in. thick, and, if there is the proper amount of metal in the cap, it should, when expanded, be about 1 in. or 1¼ in. larger than the greatest diameter of the shell. I think this is the reason why the shell will go through whole. If you get a cap that is too small, it does not expand to the diameter of the shell at the shoulders, and in nine cases out of ten the shell will break up, although it may go right through the plate. I am very glad to see here the cap which Mr. Hadfield has brought, as it fully bears out my theory. The pieces of cap I have picked up have usually been very similar to that of Mr. Hadfield, but they are more like the accompanying illustration. They are, of course, not so perfect as his, as the capped shell trials I have carried out have been against hard-faced armour plates in every case. These pieces have invariably been about ¾ in. thick for a 12-in. shell, and about ⅝ in. for a 6-in. shell, and I have always noticed that the inside face, marked A, has a diameter equal to that of the shell at the shoulders, so that, if you could pick up enough pieces and put them together, you would get a complete ring similar to the one Mr. Hadfield has brought. I do not think that these caps which Captain Tresidder refers to, and which have had a blow from a tup equal to from 25 to 30 foot tons, are in any way analogous to those that have been subjected to what happens when the shell hits the plate. You must remember that the shell is going at a very great velocity and is striking with an energy of somewhere about 2,000 foot tons, and the cap receives a much greater and altogether different blow from that of the shells which have been standing on their bases while the tup has been dropped on them.



The President: I am sure, Gentlemen, you will agree that I should move a vote of thanks to Captain Tresidder for his most interesting paper, and that he should have an opportunity of replying in writing to the criticisms made on his paper to-day.

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Written Contributions to the Discussion.

Major M. E. Edwards, R.A.: There are one or two points which occurred to me in connection with this paper. As regards shape of cap, if the function of the cap is solely to give lateral support to the point, then, as regards armour attack, the flat cap flush with the point would, weight for weight, conceivably be better than any other shape. On the other hand, there is, as you say, at least the possibility that the surface of the plate may be slightly weakened by the cap before the actual point of the projectile impinges on it, and there is, I think, another and important function of the cap, viz., that it serves as a buffer or cushion to the point, thus reducing its liability to become pulverised by the very suddenness of the shock of impact. Lieut. Jones, of the A.O.D., in writing to me once on the subject, objected to the lateral support theory because he said the shock of impact was comparable to that of a high explosive detonated at the crown of an arch, in which case the effect produced on the latter was independent of the lateral support of the haunches. I thought that there was something in his argument, if it were conceded that the two kinds of shock were strictly comparable; but, as rapidity of application of shock was the essence of the comparison, it seemed to me that the buffer action of the cap made all the difference in allowing the point to get full benefit from the supporting ring. Another and important objection to the flat cap is, I think, the effect it would probably have in unnecessarily adding to the air resistance and so reducing the striking velocity below what it would be if a long ogival-shaped cap were used, perhaps making all the difference between the projectile striking below instead of above the second critical velocity.

Then, as regards the figure of merit of plates, there is, I think, no doubt that in the case of a punching as opposed to a boring attack—*i.e.*, with uncapped projectiles—or with capped ones below the second critical velocity, or with any existing type of common shell, the figure of merit falls off very much as D becomes greater than *t*. When, therefore, we consider the practical problem of attack, and realise that the striking velocity will be oftener below 1,800 ft. sec. than over it, and that the angle of incidence will seldom be less than 30 deg., it becomes evident that even greater value attaches to the bigger shells than that due to their superior perforating—as opposed to punching—power (relatively, however, less than that of the smaller shell) and to the fact that they lose their velocity less rapidly than the smaller shell.

Major Strange, late R.A.:—My experience as Government Inspector and since I have been in the employ of Messrs. Thos. Firth & Sons, leads me to entirely concur in your proposition that the lateral support given to the point of the projectile by the cap is the most important function which it performs. I think, however, that it also has two subsidiary effects—that of buffer, which I have previously mentioned to you, and also the disrupting effect in flaking off the hard skin of the plate by penetrating into the circumferential cracks which are formed in the plate (between the hard face and soft back) by the shell as it breaks through the hard face. The effect of "fluid pressure," developed when a soft but almost incompressible material (such as the cap) receives a blow delivered at high velocity from another body, is extraordinary. The phenomenon of a shot gun blowing off the muzzle when blocked by earth or clay is well known to you. An experiment I once carried out on somewhat similar lines gives further light on this subject. It was my duty, on a certain occasion, to endeavour to ascertain the causes of ·303 rifle barrels bulging very badly when in the hands of the troops. We thought it might be caused by an accumulation of cordite, or what was then called "smokeless blank"—that is, cordite without vaseline—and we made many endeavours to bulge or burst a barrel by blocking it with explosives, trying every explosive we could think of, including fulminate of mercury, but we were unable to produce any serious damage. It then occurred to me to try fluid pressure. I took a Martini-Henry barrel, placed a lead bullet part of the way down the bore, and then put a ·303 rifle opposite it, muzzle to muzzle, and fired the ·303 bullet into the Martini-Henry barrel. At the point where the Martini bullet was lodged the barrel was cut off all round almost as if it had been done with a knife, showing the

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enormous local fluid pressure developed by the sudden arrest of the ·303 bullet travelling at high velocity. I then put a bullet in the bore of a ·303 rifle and fired another bullet into it. This reproduced exactly the bulges shown in the barrels in the hands of the troops. The fluid pressure, therefore, is evidently far greater than that set up by a charge of fulminate. We had suspected that this might be the cause, but all our endeavours to lodge a bullet in the ·303 barrel had proved ineffective; hence our experiments with explosives. If any charge in a cartridge was sufficient to move the bullet at all, it always carried it through the barrel. There is no doubt that maximum stress intensity is the main consideration in deciding the question as to whether a shell or plate gains the day. Observation leads me to believe that if the shell will hold together until it has penetrated 3 in. or 4 in. into the plate, the volume of stress intensity becomes great enough to shear a plug out of the ordinary plate. The problem is, therefore, as regards projectile manufacture, to make the projectile hold together until it has penetrated to this extent. The stress intensity diagrams shown in the paper are most interesting and valuable.

Mr. J. S. GILLINGHAM, R.C.N.C.:—The investigation of the author's formula for the penetration of homogeneous unbacked plates given by Captain Tresidder in the Appendix to his paper, being presumably in the nature of a "justification" for that formula, I should like to call attention to the mathematical argument on page 39, especially in view of the statement given therein that because a certain law holds good for *certain particular values* of the relation $\frac{D}{t}$, therefore that law must hold good for *all values* of $\frac{D}{t}$. Following the argument put forward by Captain Tresidder, put $\frac{D}{t} = x$ in equation (2) (x being necessarily a variable, according as the ratio of calibre of projectile and thickness of plate varies), we get

$$t \text{ varies as } \frac{W V^3}{D \cdot t (1 + 3x)};$$

or, writing y for $\frac{1}{1 + 3x}$,

$$t^2 \text{ varies as } \frac{W V^3}{D} \cdot y; \qquad (3)$$

and since x varies with every different value of $\frac{D}{t}$, therefore y also must vary with every different value of $\frac{D}{t}$. Hence, converting (3) into an equation

$$t^2 = C \cdot \frac{W V^3}{D} \cdot y$$

where C is a constant and y is a variable. This, I think, makes it clearer why Captain Tresidder's so-called "constant" requires to be varied for different values of the relation $\frac{D}{t}$—a little clearer, perhaps, than is suggested on page 40 of the Appendix. I am inclined to think that the formula is better arrived at by equating "Work" with "Work," as suggested by Captain Tresidder in footnote on page 39, as one is then better able to grasp the various mathematical assumptions made.

Captain T. J. TRESIDDER, C.M.G., sent the following written reply:—I have first to thank Mr. C. E. Ellis for so kindly reading my paper for me in my absence. With his opinion that, for all-round resistance to various kinds of attack, the face-hardened Krupp plate is the best of which we have any present knowledge, I quite agree. Mr. W. E. Smith rightly points out the great cost usually attending armour plate trials; but I would suggest, firstly, that there are a few experiments for deciding main principles that would justify almost any expense; and, secondly, that the Government are in possession of facilities—not available to private firms—for carrying out many most instructive tests at a comparatively trifling cost. In Germany, it is understood, exhaustive trials have been made as to the best form and size for caps. We cannot expect to be allowed to profit by this experience, and, if we are to have the knowledge, we must make experiments for ourselves. There are a host of other points, some involving

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large expense, some very little, which should form the subject of experiments made systematically and exhaustively—each with one, and only one, object in view—under the direction of an officer thoroughly conversant with the subject, and who would not have numerous other duties to perform.

In reply to Mr. R. A. Hadfield, by "plates having toughness throughout" I meant those that were neither cemented nor face-hardened. Certain qualities of plates that are not cemented can be face-hardened, but cementation without hardening I should expect to be highly mischievous. There can be no question that tough material, cast into shape, such as is manufactured by Mr. Hadfield's firm, is a most valuable auxiliary to forged and hardened armour, being adapted to several purposes where the use of the latter is not practicable. In quoting from the top of page 28 of my paper, it has, apparently, escaped Mr. Hadfield that by an "all-hard" plate I did not mean a K.C. plate, but a plate that was throughout its thickness like the front of a K.C. plate. Such a plate, I think I was right in stating, cannot be made so that it will not crack under heavy impact. It was alluded to as the hypothetical plate that pinned all its faith to F and neglected S.

I see I have also failed to make myself clear to Mr. Hadfield on the subject of the "first critical velocity." He seems to deny the existence of such a velocity because his projectiles did not illustrate it at 1,000 f.s. I would point out that he did not go low enough. In the case of the 4·7 in. Krupp shell and the 6-in. Harveyed plate (quoted on page 31 of my paper) the shell had the advantage of small calibre and very excellent quality, while the face of the plate was far less hard than that of a modern K.C. plate. I expressly stated that "against a modern Krupp plate, with a proper depth of hardness, the first critical velocity would probably be very much lower." Mr. Hadfield did not find it with his projectiles at 1,000 f.s., but that is no argument against its existence. Perhaps he would not have found it at 500 f.s. I cannot say. But if not, and he had continued reducing the velocity, somewhere between 500 f.s. and zero it would assuredly have been found. With regard to the double-ended cone alluded to on page 30 of my paper, Mr. Hadfield says he does not always find that cone occur, but has seen it sometimes. This is positive evidence in support of my statement and only negative evidence against it. I take it that the principle governing the pulverisation of a projectile's point on impact with a hard face is identical with that governing the pulverisation of a punch's point under the same circumstances. In the former case, however, destruction of the testifying fragments is usually so thorough that it is to be wondered at that their evidence is ever available, even occasionally; in the latter case the violence of the collision can be modified with nicety till the destruction can be detected in the act of its commencement. In such cases I have found the double-ended cone, not generally only, but invariably.

Next, as to experiments with "pressed" caps (although I used pressure only to enable me to calculate the force of blow necessary, and then did the crushing exhibited with a single blow on a fresh cap), Mr. Hadfield, speaking of earlier experiments of his own, says: "We got a little further than any of the caps shown here, and absolutely made the hole." Now, the object of my experiments was to find out something about the work the plate would have to do in crushing the cap *before the shell arrived.* Once the point of the shell shows through the cap *the shell has arrived.* I took the greatest care to so regulate the blow as to complete the crushing *to* the point of the shell *and not beyond it.* I wanted to ascertain the "work" the plate is called upon to do while the shell is, so to speak, "soft-nosed"; for afterwards the cap is, most certainly, little more than a lateral reinforcer, while previously it is a "buffer." While acting as a buffer, the cap calls upon the plate to do the work (F S) of crushing it. If the plate, having too much hard face, or some other reason for excess of rigidity, tries to do this work with a minimum of S and a maximum of F, the pressure expressed by the latter may exceed the strength of the peripheral attachment of the attacked disc to the rest of the plate, and then that disc just retires bodily in advance of the projectile. This sort of perforation is very common. It is not the work of a capped shell (in the ordinary sense) but that of a soft-nosed one; and a cheap soft-nosed projectile would probably perforate equally well. But if the plate does not attempt a hopeless rigidity of resistance, and puts a reasonable amount of S into its F S, then the attacked disc does not retire at so early a stage, and the shell itself arrives and finds the face of the plate either intact or partially

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disintegrated. It was to discover which of these two conditions was the more probable, as judged by the amount of work involved in the crushing of the cap to the shell's point, that was the object of my experiments. My conclusion, as stated in my paper, was that, at ordinary striking velocities, the plate's face would not usually be disintegrated before the arrival of the shell itself. I could not wish for a better justification of that conclusion than Mr. Hadfield's interesting statement showing that on two occasions at least within his experience the disintegration of the plate's face must have been subsequent to, and so could not have been caused by and during the deformation of the cap.

I am especially indebted to Captain Tupper for his contribution to the discussion, as direct evidence on such points as that on which he touches is very difficult to obtain and proportionately valuable when it is forthcoming. There seems to be some difference of opinion among practical gunnery experts, however, on the subject, and this I attribute to varying conditions in the cases that have been observed. Each case must be dealt with on its merits. The heavy 12-in. projectile, with high velocity of rotation, is a very powerful gyrostat, while a small howitzer shell, fired at high elevation and low velocity, is a comparatively weak one, and might be expected to behave differently even if the relative position of its centre of gravity and centre of figure did not differ from that of the 12-in. shell. An elongated projectile in flight may be compared to a weather vane which should always point in the wind's eye. For this purpose the vane has its fixed axis of rotation situated well forward of its centre of figure. The analogous axis of rotation in the case of the free projectile must pass through its centre of gravity, and unless its centre of figure is well behind this its "weather-vane" action will be feeble. The wind in whose eye it should always point is the relative wind caused by its own passage through the air. An elongated projectile fired from a smooth-bore gun would require to have its centre of gravity impracticably far forward of its centre of figure to prevent it tumbling over and over. Therefore, rifling is necessary to impart to the projectile a tendency to keep its long axis in the same direction as that in which it leaves the muzzle. This is much better than letting it take any direction it likes; but it is not what we want. What we want is for the long axis to keep continuously coincident with the tangent to the trajectory of the shell's centre of gravity. Too high a speed of rotation—too powerful a gyrostatic action—precludes this, and is, therefore, most pernicious. This speed of rotation is dependent on two things—muzzle velocity and pitch of rifling. When we design new guns with increased muzzle velocity we seem to forget that we ought to reduce the pitch of the rifling to avoid unintentionally increasing at the same time the speed of axial revolution, which, if my argument, confirmed by Captain Tupper's observation, is correct, is already far too high in our most important naval guns. We seem to adhere to one turn in 30 calibres. According to the *Naval Annual*, the German and Italian pitch of rifling in the most important naval guns is one turn in 50 calibres, which is no doubt ample. This is a point of very great importance, and would well repay careful investigation.

In reply to Mr. Coast's remarks, in my own opinion it is quite satisfactory to treat S, the distance of arrest, as directly proportional to t; but it has been objected that this leaves the projectile's length out of account. I therefore made, in the argument, the change which Mr. Coast notices, so as to bring in L. He asks: "Why cannot all the D's be expressed in terms of t?" The answer is that that could be done, but it would lead nowhere. Everyone has met the tricky algebraical equations which defy solution till one particular set of steps is adopted. All sorts of other justifiable steps might be taken, but, if they leave the problem unsolved, the fact that they are justifiable is irrelevant. To the criticism, "Captain Tresidder's formula does not help us much. . . . We do not seem to be much nearer getting a constant co-efficient than Fairbairn was," I would reply, Fairbairn's was a formula for wrought iron; mine is a formula for wrought iron. Dealt with as such, Fairbairn's required a change of co-efficient for every substantial change of velocity; mine does not. This is not only a radical improvement, it is a change from the unsound to the sound. The fact that by an intelligent empirical selection of the one co-efficient N, we can make the formula serve for all qualities and thicknesses of plates, all diameters and weights of projectiles, and all variations of velocity, ought to be held at least an advantage, if not a confirmation of soundness of principle. With regard to the letter quoted on page 15 of my paper, I regret to be unable to add to the information contained in the letter itself.

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Mr. Samuda Benthall, in confirming some of Mr. Hadfield's observations, adds practical confirmation of my view that the capped projectile tends to force its way through the cap by stretching it like a garter. It is to the lateral support afforded by the garter in its resistance to stretching that I attribute the projectile's immunity from pulverisation on impact. With regard to Mr. Benthall's criticism as to the difference between my falling-weight experiments and the blow received by a projectile on impact, I should like to say that, before making these experiments, I very carefully considered how far I should be justified in basing an analogy on them. I came to the conclusion, and still think, that the analogy is quite good so long as no confusion of ideas is permitted between "work done" and "power exerted," and the fact is not lost sight of that no material is capable of transmitting strain-energy from its surface inwards at unlimited speed. If we raise a weight of one pound against the action of gravity to the height of 100 feet, we do exactly the same amount of work—100 ft. lbs.—whether we spend a second or an hour in the operation, although the "power exerted" is 3,600 times greater in the former case than the latter. Similarly, if we crush a steel cap by slow pressure, or more rapidly by the blow of a falling weight, or still more rapidly by ballistic impact, or most rapidly by putting a plate on its point and detonating a high explosive on the plate, we do precisely the same amount of work on the cap provided *the rearrangement of its molecules is the same every time.* That rearrangement may be expected to be approximately the same as long as the rapidity with which the strain-energy is applied at the surface does not exceed, or too nearly approach, the greatest rapidity with which the material can transmit it from the surface inwards, This excess rapidity of communication of strain-energy (which spoils the analogy) will certainly be reached in the case of the high explosive, but in the case of ballistic impact at current velocities I do not think it will be; and some idea of the energy absorbed in crushing the cap to the shell's point by ballistic impact may be obtained by observing the energy required for the same purpose under a falling weight. The conclusion pointed to—that the stress-intensity is insufficient in ordinary cases to disintegrate the plate's face before the arrival of the shell itself—is notably confirmed by Mr. Hadfield's observations recorded in this discussion.

The remarks of Major Edwards carry special weight from the fact that, besides being a diligent student of the subject, he occupies a position conferring unusual facilities for acquiring experience. The "buffer" action of the cap I have already alluded to, but the idea it conveys to me is very different to that held by Major Edwards, which I understand to be that the deadening of the shock on the projectile due to the interposition of the cap is what is favourable to the former. Defensively, of course, it is so; but the projectile's rôle is offensive. You use a raw-hide mallet instead of a steel hammer when you do not want to bruise the material struck; not when you do. The projectile's first object is to get through the plate. Its weapon is its shock power; it does not want to deaden that. If the cap deadened the shock on the shell only and not on the plate, then only its buffer action would help the former relatively to the latter. But this is impossible. In Major Edwards' comment on Lieut. Jones' analogy between ballistic impact and the blow delivered by a high explosive on detonation, I quite concur. Striking velocity would have to be many times higher than any known gun can impart before this analogy would hold, and then we should come to a condition of things when (imagining a plate to consist of numerous thin hypothetical layers) the strain-energy communicated to the first layer would cause its failure before it had time to call on the next layer for its assistance, and the second layer would fail before it could obtain the support of the third, and so on. Each layer would fail in succession seriatim under a pressure proportional to its tenuity (which would increase with velocity) just as four $\frac{1}{4}$ in. plates, or ten $\frac{1}{10}$ in. plates, punched separately, only require a pressure on the punch of respectively one-fourth, or one-tenth, that required for a one inch plate. This is the theory, which I call the "Seriatim theory of high-speed punching," alluded to in footnote No. 8 of my paper. It argues that, after perforation velocity is reached, further increases of velocity do not increase pressure between plate and projectile, but reduce it; and this is why excess velocity always helps the shell to hold together, and helps the candle fired through a board to avoid deformation. Major Edwards' objection to the flat cap—that it would increase air-resistance—will be surmounted by providing it with a pointed nose of soft wood or a light hollow steel false point. Major Strange's explanation of the cap's action as being in the nature of

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fluid pressure would, I think, be more convincing if it were certain, or even probable, that the lateral spreading of the cap took place after the shell's head was embedded in the plate. My own view, of which Mr. Hadfield's remarks offer strong confirmation, is that (except when the attacked disc of the plate yields in shear while the compression of the cap is going on, and before the shell itself arrives—that is when the projectile's hard sharp point takes no part in the conflict, and it might as well be soft-nosed) the cap remains outside the plate till it has stretched enough to let the projectile through; after which the whole or part of it may be carried into the plate if it has not rebounded earlier, as described by Mr. Hadfield. The deduction which Mr. Gillingham questions, though correct enough, I admit I did not demonstrate to be inevitable. I gained a little brevity, but laid myself open to this criticism which is strictly justified. The reason for not arriving at the formula by equating "work" with "work" was, as explained in my reply to Mr. Coast, to satisfy the hypercritics who insisted that the length of the projectile must be taken into account.

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MODERN TORPEDO BOATS AND DESTROYERS.

By J. E. THORNYCROFT, Esq., Member.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 8, 1908; the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D., President, in the Chair.]

SINCE the introduction of the torpedo-boat destroyer rather more than twelve years ago, such changes have taken place in their construction, and the torpedo boat itself has been enlarged to such an extent, that a review of the developments may be of some value.

Considering the vessels of the British Navy, it will be found that the largest torpedo boats were vessels of about 130 tons displacement and 2,000 I.H.P., developing a speed of 23 knots. They were, with one exception, single-screw boats, and were, except in a few cases, fitted with locomotive boilers. The majority of the boats were considerably smaller, and only had a speed of about 20 knots. The only watertight compartments were formed by the bulkheads—watertight flats in the compartments at the ends of the vessels not having been at that time adopted. The most powerfully armed were fitted with one bow and two deck tubes for 18 in. torpedoes, and three 3-pounder guns.

At the same time the French and German navies possessed a large number of torpedo boats of about the same size and power, but differing greatly in design from the British boats. The French design of boat came primarily from the yard of Normand, and the German from that of Schichau. In addition to their torpedo boats, the German Navy possessed a number of much larger and more powerful boats, which were called "Division" boats. These were employed with the torpedo boats, and carried the commanding officers of each division. Although they were vessels of considerable tonnage, being over 300 tons, they did not develop a much greater speed than the torpedo boats.

The history of the inception of the torpedo-boat destroyer in the British Navy is so well known as to require only a passing reference. The efficiency of machinery which had been fitted to the torpedo-gunboat class being insufficient to enable them to develop a higher speed than the torpedo boats, the British Admiralty decided that vessels of the torpedo boat type should be built of sufficient power and dimensions to ensure their being able to always overtake and destroy torpedo boats with the guns they carried.

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The first vessels of this new class, which were called torpedo-boat destroyers, were built by Messrs. Yarrow (*Havoc* and *Hornet*) and Messrs. Thornycroft (*Daring* and *Decoy*). Their trial displacement was about 240 tons, and the power developed was over 4,000 I.H.P., which gave a speed of 27 knots. They had a high freeboard, were excellent sea boats, and were able to maintain their speed in rough weather. Although in dimensions they were somewhat smaller than the German division boats, their greatly increased speed gave them a much higher fighting value, and they were considered such a success that forty more were put in hand during the next two years. It was the introduction of water-tube boilers in these vessels which primarily enabled this high speed to be obtained, and their working was so successful that they were at once adopted by every builder of this class of vessel.

During the next few years the experience gained with them when keeping at sea for considerable periods, made it desirable to increase the size and power, and six years after the first boats were built, the new vessels that were being put in hand had been increased in size by about 100 tons, and the power from rather over 4,000 to 6,000 I.H.P. Their strength and safety had been very much improved by the introduction of watertight flats in the end compartments. The bow torpedo tube had been dispensed with, and their speed had been increased to 30 knots by the employment of high tensile steel in their hulls, and improvements in their machinery. The earlier vessels were constructed of steel of the character usually employed in ship construction, but the high grade steel employed in these later boats had a tensile strength of between 37 and 43 tons, and was so tough as to show an elongation of 15 per cent. on an 8 in. test piece. The employment of this steel was a very important departure, as it enabled 15 per cent. of the weight to be saved in the structure of the hull. The stresses in the hull were about 9 tons in tension and $7\frac{1}{2}$ tons in compression. Perfectly satisfactory results were obtained, and trouble was only experienced in some isolated cases where local stiffening or compensation had been insufficiently considered. During manœuvres, and in making long passages, some instances occurred in which it was thought that greater strength might be advantageous, but the damage sustained was not greater than might have been expected to happen to any vessels under similar circumstances. The fact that all the Japanese destroyers built by British firms made the voyage to the Far East, were at once put into service without any repairs, and went through the war without developing any structural defects, proves that this class of vessel was of ample strength.

After the British Navy had taken the lead, we find that the more important foreign navies quickly followed its example. The new division boats in Germany were built with water-tube boilers, and at once became available as torpedo-boat destroyers. There is, however, considerable difference between these vessels and the British, the German vessels having a very much lower freeboard, and drawing more water, besides being fitted with a submerged bow tube.

Mr. Ziese says that the attention of his firm (Messrs. Schichau) is specially directed to designing their vessels to be as low as possible in the water to reduce them

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to the minimum target. It will be seen from the comparative designs (Plate IV.) that the typical German vessel (Fig. 3) is certainly much less prominent in this respect than the later British vessels (Fig. 4). The German vessel is a good deal finer forward, and the greatest beam is placed further aft; the stern is deep and narrow, quite unlike the broad, flat British sterns. The bridge is kept back as far as possible, being quite close to the forward funnel, one or more deck tubes being placed in front of it, and behind the low turtle back. This is, no doubt, a good position for them from a control point of view, but it is thought doubtful if they can be so efficiently worked in bad weather as the British vessels, with their greater freeboard and the torpedo tubes aft.

The destroyers built in France (Figs. 1, 2, and 5) have also a very low freeboard compared with the British boats, and possess quite a special construction of hull. The late Monsieur Normand, and some of the leading French naval architects, were of opinion that much greater strength and security in bad weather are obtained by constructing a vessel with a turtle-back form from one end to the other. This form originated with the Normand torpedo boats, and was developed to such a degree of exaggeration in the French destroyers that a platform or hurricane deck had to be provided to enable the crew to work the guns and get from one end of the vessel to the other. The hatchway coamings, skylights, &c., are all brought up to this platform, so that the seas can wash over the deck of the vessel—the crew standing above the water. No doubt a considerable amount of weight may be saved by this construction, but it is doubtful if the vessel can be so efficiently worked in bad weather as one with a much greater freeboard. It must not be forgotten that a type of boat may be suited to one class of sea and coast, although not successful in another, and the French type may be all that is required for the Channel, but would not do so well for the North Sea or the Atlantic. Monsieur Normand has informed the author, however, that in the latest destroyers of large dimensions which he is building he has abandoned this special construction, and has adopted much more the British type, as will be seen from Fig. 1, which he has kindly supplied, showing one of his latest vessels. It is understood that this change is not in any way because the construction is unsatisfactory, but simply that it is less suited to larger vessels, and is more costly. In support of the advantage of what may be described as the British type of vessel, it is interesting to note that besides the Japanese, most navies that have built destroyers have adopted British designs. The British Naval authorities have considered the efficiency of destroyers in bad weather to be of such importance, that in 1901 it was decided that, in the new vessels, speed should be reduced to the extent of 4½ knots, to obtain better sea-keeping qualities, and greater strength of hull, and more comfort for officers and men. These vessels were known as the "River Class" (Fig. 4, Plate IV.), and while opinions differ as to their fighting value compared with that of the earlier 30-knot boats, it has been conceded that the reduction of speed was too great. There has been no falling off in the demand for high speed in vessels built by foreign navies, and the latest destroyers built for the British Navy have been designed for a speed of not less than 33 knots.

The River Class destroyers have a displacement of 525 tons, and develop about

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8,000 I.H.P., which gives a speed of 25¼ knots. Not only are they of greater freeboard and beam, but have a high forecastle, the deck of which is about 14 ft. above the water. This makes them very comfortable and roomy boats, but, of course, much more conspicuous. Vessels with hulls of this type are now being built for a foreign navy, where they will be required to work in very open water, and they have also been proposed for the Australian Commonwealth, where similar conditions prevail. The officers who are responsible for this decision have also made the proposal to put the officers' quarters in the fore part of the vessel. The suggestion has often been considered before, but could not be carried out satisfactorily without the extra room provided by the double forecastle.

Reference has been made to the kind of stern adopted in the majority of the German vessels being different to what has been described as the "British type." The form of stern necessarily largely determines the type of rudder that can be fitted. As in other matters, there appears to be considerable difference in opinion as to the amount of steering control which is necessary. The diagram Fig. 6 (Plate IV.) shows a broad stern and double rudders as fitted to the first of the destroyer class *Daring*, &c., which was copied from the earlier Thornycroft torpedo boats. The arrangement of two rudders, on either side of the screws, probably gives the best possible steering qualities both ahead and astern, but has the disadvantage of the complication of two rudders.

It will be seen that the German type of rudder, as fitted by Messrs. Schichau, is very much more of the ordinary cruiser type of balance rudder. In some of the fastest of the French boats the rudder has been fitted forward of the screws, but in the later vessels it is arranged like the British boats, with the rudder spindle outside the stern of the vessel. Opinions differ very much as to the advisability of protecting the rudder, which may be done by placing it under the stern or by means of an overhanging counter, but if either of these methods is adopted it is much more troublesome to unship the rudder, than when it is placed clear of the vessel.

The difficulty of steering astern with a single-screw boat is thoroughly understood, and on this account alone there is a strong argument in favour of twin screws for all torpedo craft. The speed at which torpedo craft should be required to be controlled astern is one which must be fixed by naval officers. While the very best stopping power will be recognised as of the first importance, it is thought questionable if the steering gear should be required to control the vessel at more than 20 knots astern.

The new 33-knot type of destroyer, of which five have been built, and seven are in course of construction, or about to be put in hand, are of between 800 and 1,000 tons displacement, and are nominally of about 15,000 I.H.P. Experience gained with the experimental vessels *Albatross* and *Express*, intended for a speed of 32 knots, seemed to show that with reciprocating engines it would be very difficult to develop higher speed, and therefore in these new vessels, where 33 knots were demanded, it was decided, in view of the favourable results that had been obtained with Parsons turbines in the several torpedo-boat destroyers in which they had been tried, to adopt

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them for the new vessels. It is now generally known that, in conjunction with oil-fired boilers, the turbine engines have given results which could not have been obtained without their adoption. The *Tartar*, the fastest of the class, has maintained a speed of 35·36 knots on a continuous run of six hours, or practically 10 knots more than the River Class could maintain on a four instead of a six hours' trial. While giving every credit due to the turbines and oil fuel for the part they have played in enabling this result to be obtained, it will be agreed that the greatest credit is due to Mr. S. W. Barnaby and the other officers of the firm who have been responsible for the design and building of the vessel. The increase in size over the earlier types which has been found necessary to enable this speed to be obtained with the required radius of action has been very considerable, the length in the case of the *Tartar* being 272 ft. as against 225 ft. in the River Class, and the displacement nearly 900 tons instead of 600 tons. It is worth noting that the strength and proportions of all the individual parts have been kept up to the standard of the River Class, and the stresses in the structure of hull do not exceed 8 tons in tension and 6½ tons in compression, these being the limits which have been laid down by the Admiralty as necessary for this type of vessel.

Apart from the coal capacity of a destroyer, the duration of time for which full speed can be maintained depends on the length of time it is possible to run without cleaning the fires, which at full speed, with average coal, is not more than three or four hours. With liquid fuel, stoking is reduced to a minimum, and full speed can be maintained as long as the fuel lasts. The importance of the most skilful attention to reciprocating engines is well known. With turbines, both the number of men and the closeness of attention are very greatly reduced, and it is considered that the difference which has been made by the use of turbines and liquid fuel in these vessels has had as great an effect as the introduction of the water-tube boilers in the first destroyers. The extent to which destroyers of this power will be adopted by foreign navies is uncertain in view of their very great expense, and in their programmes it will be found that vessels of what may be described as an improved British 30-knot class are being adopted.

While the destroyers have been shown to have increased in size and power very greatly from the original type, the torpedo boat has developed almost to an equal extent. In each succeeding order for the British Navy torpedo boats have been made rather larger than their predecessors, until the last reciprocating engine boats ordered in 1903, which were of 200 tons displacement and 2,900 I.H.P., gave a speed of 25½ knots; they carried three 18 in. torpedo tubes and three 6-pounder guns. The latest types of torpedo boats were ordered in 1905, and were at first called "coastal destroyers." They carried the same torpedo armament, were of slightly greater dimensions, but were fitted with turbines and oil fuel, and as far as power and speed are concerned they are practically the same as the first 27-knot destroyers. The adoption of oil fuel, however, has given them a much greater radius of action at full speed.

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While the turbines have given excellent results in these vessels, it is a question if equally good results would not have been obtained with twin-screw reciprocating engines and oil fuel, as while the merits of turbines for larger vessels are admitted on all hands, there is considerable doubt, when powers of less than 3,000 or 4,000 I.H.P. are required, if the greater simplicity of reciprocating engines is not to be preferred. The necessity of adding a cruising turbine, and, owing to the high speed of revolution, of adopting at least three shafts, makes it extremely difficult to arrange the engine-room satisfactorily in such small vessels. No doubt with experience it will be found possible to simplify to some extent the arrangement of pipes and auxiliary engines, but in their present form these adjuncts amount to so much, that the fitting of the machinery on board becomes an extremely costly matter, and when it is necessary to open up, an enormous amount of work is entailed. It is thought that it will always be found necessary to put these vessels in dockyard hands if any adjustments have to be made to the machinery, as it will be quite impossible for the crew themselves to deal with adjustments without greater facilities than can be provided on the vessels.

It is believed that it will soon be recognised that the procedure which has always obtained with regard to opening up the machinery after the contractors' trials, and the periodical opening up for inspection, should, with turbine machinery, be discontinued, as not only are so much labour and time wasted, but considerable risk occurs every time the engines are opened up. When once they have been properly adjusted there is not the same reason for examining the rotors, &c., that exists for looking at the pistons and slide valves of reciprocating engines.

The arrangement of turbines that Messrs. Parsons have thought best for smaller powers, from considerations of simplicity and lightness, is that of three shafts, viz.: the high pressure turbine on one wing shaft, the intermediate pressure turbine on another, and the low pressure turbine, of more than one-third power, on the centre shaft. As the reversing turbine is necessarily a part of the low pressure turbine, only one shaft is available for astern going, so that from the control and steering point of view, the vessel is practically a single-screw one. Some of the earlier turbine vessels did not have very good astern power, but in the later torpedo boats, and particularly in the 33-knot destroyers, the astern going power has been enormous, the latter vessels being capable of making upwards of 25 knots astern.

In the Italian Navy there are excellent examples of torpedo boats of 26 knots speed, fitted with twin-screw reciprocating engines, a considerable number having been built by the firms of Messrs. Pattison and Messrs. Odero, from designs supplied by the author's firm (Plate V.). They are armed with three 18 in. deck tubes, and three 3-pounder guns. Although they are vessels of under 200 tons displacement, they are sufficiently good sea boats to withstand heavy weather. For some years all the torpedo boats for the Italian Navy have had their coal bunkers arranged so that they may be used for oil tanks when required. Coal protection to the machinery, however, has been

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considered of such importance that liquid fuel has so far only been adopted to a limited extent.

The three deck tubes are arranged in the same way as has been adopted in the German Navy, viz., one aft and two forward in front of the bridge and behind the turtle back. It will be noticed that the vessel has a straight keel of sufficient length to enable it to be docked without supporting the overhanging ends. This is a feature which is found most convenient, and has been generally adopted in modern vessels in place of the "cambered" keel fitted to the early torpedo boats. The screws project very little below the line of keel, which is also a good feature where there is any likelihood of the vessels taking the ground. The possibility of vessels frequently taking the ground in certain navies has been considered of such importance that torpedo boats have been fitted with a false keel in order that the propellers may be protected. The heel piece which it was at one time thought desirable to fit under the propeller itself, being found worse than useless, has now been entirely abandoned. This false keel must, however, detract materially from the speed.

The particular coast and condition of service must, of course, be taken into consideration in the case of torpedo boats as well as destroyers, and it is possible that small vessels may be successful in some cases; but it is thought doubtful if, as a rule, good results will be obtained with vessels of much less than 180 tons. It is true that the latest British torpedo boats which are building are nearly 100 tons greater displacement, but they bear about the same relation to torpedo boats of other navies that the 33-knot destroyers do to destroyers of our own and other navies. While questions of cost may not be of the first importance in the British Navy, there are a number of countries at the present time contemplating the building of torpedo-boat craft where the amount to be expended on individual vessels will be one of the first considerations.

When considering the advantages of employing oil fuel for this class of vessel, its comparative high cost, and the difficulty in obtaining it, must not be overlooked. The features that strike those who are actually working the vessels most forcibly are the absence of dirt and cinders, and the saving in stokehold staff, the moving of the fuel and stoking being effected by steam pumps and pipes instead of stokers and trimmers. With coal-fired torpedo-boats it may be taken that the stokehold staff will be quite three times as great as with oil-burning boats, and while with coal-firing at full speed it is usually difficult to maintain sufficient steam, in practice it is found that oil-fired boilers are blowing off, or are on the verge of doing so, and when the vessel is eased up the boilers are under such perfect control that the safety valves do not lift. The necessity of easing down gradually to avoid blowing off with coal-fired boilers is of course thoroughly appreciated.

The evaporative value of oil may be taken as one and a third times that of coal, and while 43 cubic ft. of bunker space are required to stow a ton of coal, 38 cubic ft. of

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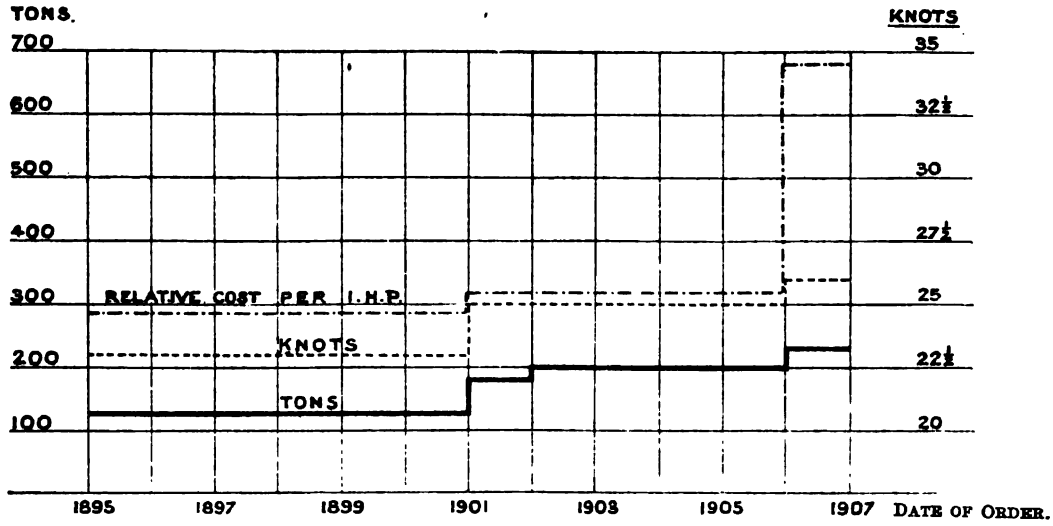
space are required to stow a ton of oil, so that the equivalent amount of oil fuel can be stowed in 70 per cent. of the space required for coal.

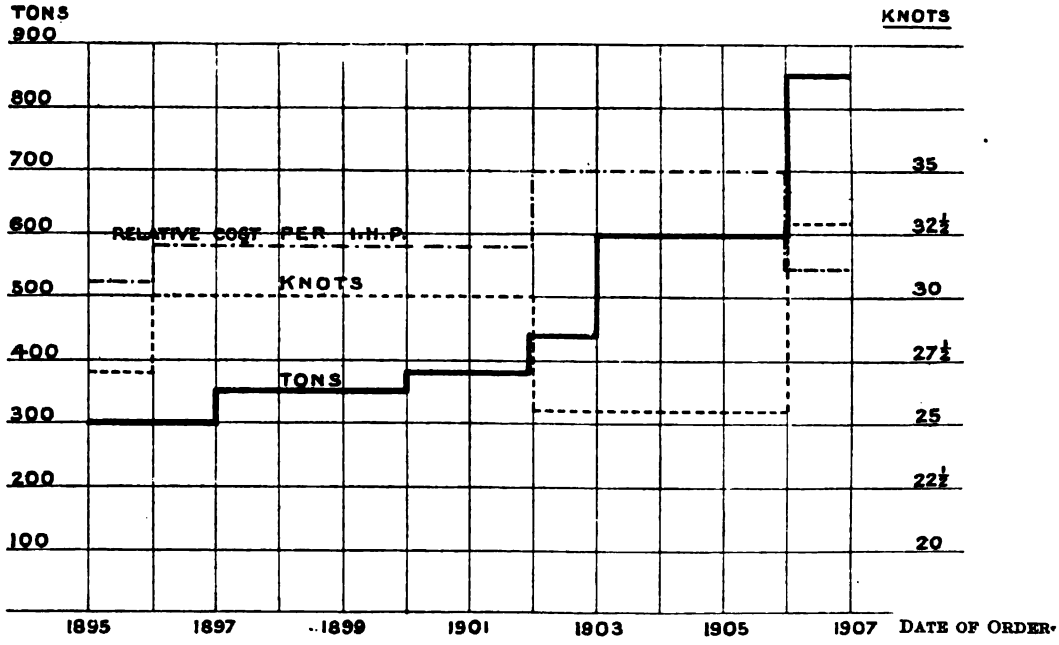
The consideration of cost is not of the first importance in fuel for warships, but it is worthy of note that the most recent oil tank steamers have not been fitted to use oil

DIAGRAM SHOWING DEVELOPMENT IN SIZE, SPEED, AND RELATIVE COST PER I.H.P.

TORPEDO BOATS.



DESTROYERS.



fuel on account of the cost, although some years ago, when Sir Fortescue Flannery read a paper on the subject at the Institution, it was expected that many vessels would soon be so fitted.

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It is interesting to compare the radius of action of the coal and oil fired torpedo boats and destroyers, but it is difficult to draw any definite conclusions in view of the coal burning boats being generally fitted with reciprocating engines and the oil-fired boats with turbines.

It is not proposed to go into the considerations of the relative economies of reciprocating and turbine machinery, as it is recognised that while the turbines have a very high efficiency at full power, at cruising speeds, even when using the cruising turbine, their efficiency is much less than that of reciprocating engines.

In the case of the torpedo boats which have been delivered to China and Japan, it has been necessary to consider their capacity to carry coal for the voyage from Aden to Colombo, and without very special arrangements it is doubtful if turbine vessels could make the voyage, while Messrs. Schichau have informed the author that the vessels which they have built for the Chinese Government actually made the voyage from Port Said to Colombo without taking any coal or water at Aden, a total distance of about 3,700 miles.

It will be noted from the diagram on page 66, showing the increase in size of torpedo vessels, that in the case of the torpedo boats the tonnage, speed, and price per horse-power have varied in approximately the same proportion, but as soon as the change to turbines takes place, the price per horse-power goes up very considerably. In the case of destroyers, the diagram shows that the price per horse-power also varies approximately in proportion to the increased size of boats until the introduction of the turbines in the 33-knot destroyers, when, owing to the very great power developed, the price per horse-power actually falls instead of increases.

It is admitted that to ensure freedom from breakdowns, and general efficiency in service with such small vessels, every possible complication must be avoided in the machinery and mechanical devices, but with torpedo boats, like all other ships, every new class seems to require something adding to increase the complication. It is thought that the time has arrived when every effort should be made, particularly where turbine machinery is employed, to reduce the number of auxiliary engines. This suggestion will be better appreciated when it is known that in the latest turbine destroyers there are 21 independent steam pumps, besides fans, electric light, and other engines.

An effort has been made by Messrs. Yarrow to attain this end in the destroyers building for the Brazilian Government, by fitting two very large boilers instead of four. The resulting simplification and economy in construction are of course great, besides a considerable saving in weight, but the advisability of using boilers in units of upwards of 4,000 I.H.P. is thought to be open to question.

The policy of some naval authorities to duplicate everything, from the main feed pumps to the syrens, is thought to be a mistaken one. The reserve pumps, or whatever piece of mechanism it may be, that are in duplicate, ought not to be required

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to be brought into operation, and in ordinary commercial engineering do not usually exist. In a battleship, which will go on fighting after receiving a great deal of punishment, the conditions may be different, but the torpedo boat or destroyer either escapes or is damaged to such an extent that no duplication will save it.

It is not within the province of the author to express an opinion on the relative importance of different types of war vessels, but it is hoped that the large number of destroyers and torpedo boats which are building or contemplated by the different Powers at the present time will be considered a sufficient justification for this paper.

DISCUSSION.

Sir PHILIP WATTS, K.C.B., LL.D., F.R.S., D.Sc. (Vice-President): My Lord and Gentlemen, Mr. Thornycroft's paper is an interesting review of the development of torpedo boats and torpedo-boat destroyers in recent years, and it is a great advantage to have such a record from the point of view of one of the most successful builders of this type of vessel. The paper discusses numerous matters of detail upon which it is unnecessary that I should express an opinion—such, for example, as the proper amount of freeboard for particular kinds of service, the best positions for the torpedo tubes, the most suitable armament for destroyers, the advantages of a complete turtle back as compared with an ordinary weather deck, and the general underwater form of the vessel at bow and stern. These features have all been determined by the Admiralty in the vessels built for the Navy, and the vessels themselves, therefore, indicate the Admiralty opinion on these points. But there are two or three points in the paper to which I wish to refer. It is not accurate to describe the so-called 30-knot destroyers as 4½ knots faster than the River Class. Under similar conditions of lading, and especially in ordinary weather at sea, these classes are practically of equal speed. Of the new 33-knot destroyers, five are nearly completed (one, the *Cossack*, has been delivered) and seven others are in course of construction. They carry a load which is between the load carried by the 30-knot destroyers and that of the River Class, but more closely approaching the latter. The decision to build vessels of this type was come to by the Board of Admiralty in the spring of 1904, and was based on the consideration of an outline design, prepared at the Admiralty, for a vessel of about 900 tons displacement. When tenders were called for in November of that year from seven of the principal firms, I found that very considerable doubts were felt as to the practicability of attaining the specified speed of 33 knots under the conditions stipulated as to lading, &c.; and, even after some discussion with the designers of several of the firms, only five of them sent in tenders and undertook to fulfil the conditions. These firms were Armstrong, Hawthorn, Laird, Thornycroft, and White. The dimensions given in the tender designs submitted by these firms were as follows:—Armstrong, length, 238 ft. 6 in. and 600 tons displacement; Hawthorn, length 235 ft. and 500 tons displacement; Laird, length 260 ft. and 650 tons displacement; Thornycroft, length 255 ft. and 650 tons displacement; White, length, 250 ft. and 540 tons displacement. An order was finally placed with each firm for one vessel. The dimensions of the actual vessels as constructed by the several firms are:—Armstrong, length 250 ft. and 860 tons displacement; Hawthorn, length 255 ft. and 865 tons displacement; Laird, length 270 ft. and 890 tons displacement; Thornycroft, length 270 ft. and 895 tons displacement; White, length 270 ft. and 890 tons displacement. It will thus be seen that considerable modifications were introduced in conjunction with the Admiralty staff. All of the designs were most carefully worked out in every detail, and were finally brought into line with one another and most of the parts were made interchangeable. The

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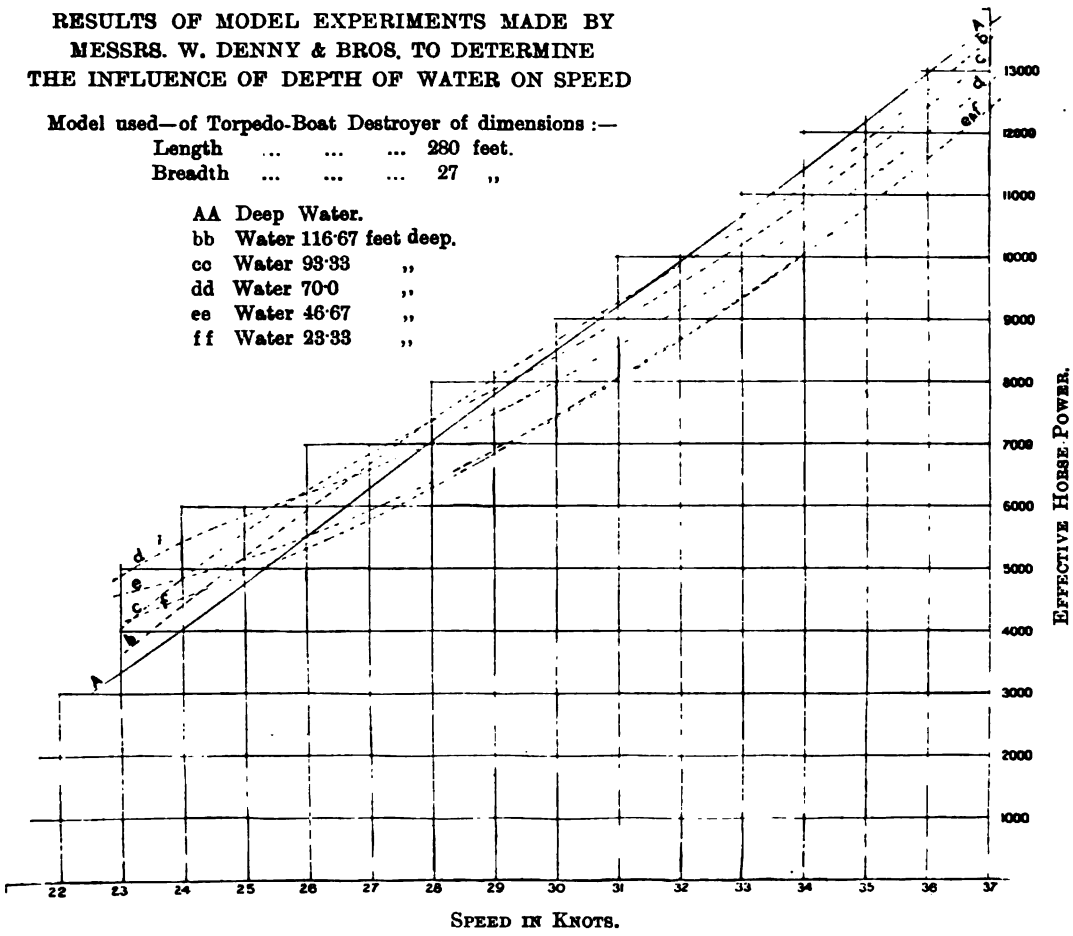
main engines, for instance, are precisely the same in each, and all their principal parts are interchangeable. All of the vessels have now been run on the measured mile. Their trials are not yet completed, but as a class they have got their speed, and the results have quite justified the original anticipations of the Admiralty.

The last point I wish to refer to is one of considerable importance, and has to do with the statement concerning the speed of the *Tartar*. Mr. Thornycroft says on page 63: "The *Tartar*, the fastest of the class, has maintained a speed of 35·36 knots on a continuous run of six hours, or practically 10 knots more than the River Class could maintain on a four instead of a six hours' trial." I think this statement



requires explanation. If he had said that the *Tartar* was six knots faster than the River Class when similarly loaded, he would not have been far wrong. The *Tartar* is, no doubt, the fastest of her class, and I heartily congratulate Mr. Barnaby and the firm on having achieved this result, which has been mainly due to the large boiler power they have succeeded in getting into the vessel; but the assertion that the *Tartar* has maintained a speed of 35·36 knots on a continuous run of six hours is misleading, because the speed was obtained by running the vessel in shallow water on the Maplins, and this fact is not stated. The speed given should be reduced by upwards of a knot. The preliminary trials of the *Tartar* were run in Stokes Bay, and a definite increase of speed was anticipated on running the vessel in the shallow water on the Maplins. This expectation was fully realised. It has been known for many years, and the facts have been recorded in the Transactions of this Institution, that while, over a great range

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of speeds for a given vessel, shallow water involves an increase of resistance and a falling off in speed, yet, on reaching a sufficiently high speed, the resistance becomes even less than that in deep water, and the speed attained is, in a corresponding degree, increased. Such a record of speed as that given by Mr. Thornycroft for the *Tartar* is of undoubted value if accompanied by full knowledge of the increase due to the special conditions of trial; but for scientific purposes, for the purpose of a record in the Transactions of this Institution, or as a measure of the capability of the vessel for her work in H.M. Service, the uncorrected figure is misleading. If facilities for running high-speed vessels of this type in deep water existed at several points round our coast, it would be very easy to prescribe that all trials should be made under circumstances in which the recorded speeds might be accepted without qualification or correction; but the only course of the kind we have at present is that at Skelmorlie. To restrict trials to this course might be acceptable to our Clyde friends, but it would have the effect of handicapping other builders of these vessels, and of putting up prices generally, and there are practical advantages in introducing as few restrictions as possible in matters of commercial importance. In the River class, the effect of the shallow water on the Maplin course was to diminish the speed, and if the speeds recorded had been taken without correction, the vessels would have been depreciated unduly, and some of the class might have failed to attain the specified speed. It was necessary either to make allowance for the effect of depth or to run the vessels in deeper water, and the latter course was adopted in these cases. In view of the increasing importance of this question, we need fuller and more exact information concerning the effect of the several available courses on the speeds recorded. We have already in our records Captain Rasmussen's paper on the Danish torpedo-boats' experiments, the valuable account of Colonel Rota's model experiments at Spezzia, and the papers contributed by members of Messrs. Yarrow's firm. I am now able, by the kindness of Messrs. Denny, to contribute for record in the Transactions the data obtained from some valuable experiments made by them only a few months ago with models of vessels approaching in form and size those we now have under consideration. (See diagram, page 69). The results of Messrs. Denny's model experiments agree generally with the data obtained from Major Rota's experiments, and have, I understand, been confirmed by actual trials with a torpedo boat. Lastly, I am glad to say that in the immediate future steps will be taken by the Admiralty to obtain by a series of comparative trials at Skelmorlie and on the Maplins, with one of the new 33-knot vessels, more adequate data for dealing with this question.

Admiral Sir CYPRIAN BRIDGE, G.C.B.: My Lord and Gentlemen, the very few remarks that I am able to make on the paper which has been read will not lead me into anything like practical intervention between Sir Philip Watts and the reader of the paper, but there are one or two things which may be said, and which I think will not be unwelcome to members of the Institution of Naval Architects; therefore I am very much obliged to your Lordship and the members here for giving me an opportunity of saying them. The first thing that I should like to say is that if there is one man to whom the introduction to the torpedo-boat destroyer is really due, it is Lord Spencer, a former First Lord of the Admiralty. He personally took a great interest in the development of this particular class of craft. I believe the torpedo-boat destroyer is the only man-of-war (I am speaking not only of the British Navy, but of every Navy in the world), of any class, from the biggest to the smallest, which was evolved from deliberate strategical and tactical experiments carried on for some considerable time beforehand. Lord Spencer was so impressed with the results of those experiments (which, of course, were not made generally known) that he took special interest in the matter, and approved the production of the torpedo-boat destroyer. The torpedo-boat destroyer was intended to destroy torpedo boats; it was not intended that she should carry torpedo armament of any kind. Mr. Thornycroft, on the first page of this paper, has summarised the history in these words: "The efficiency of machinery which has been fitted to the torpedo gunboat class,* being insufficient to enable them to develop a higher speed than the torpedo boats, the British

* The torpedo gunboat of the *Sharpshooter* class was built for accompanying and remaining with fleets at sea; what also appeared to be required was something that was not to do that, but to operate against torpedo boats starting from hostile bases within a certain range of our own ports.

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Admiralty decided that vessels of the torpedo boat type should be built of sufficient power and dimensions to ensure their being always able to overtake and destroy torpedo boats with the guns they carried." That is the exact history of the genesis of the torpedo-boat destroyer. The point that is particularly referred to in the paper which has been read to-day, and the discussion which has taken place since, so far as it has gone, is the great importance of speed in the torpedo-boat destroyer. The idea was that she should catch up and run down the torpedo boat, which might have had a considerable start of her. Therefore, the gradual and almost uninterrupted increase of speed in the class, leading, of course, to additional horse-power and great additional displacement, has been, I think one may say, progress in the right, and, indeed, in the inevitable, direction. The *Tartar*, the fastest of the class, has, under certain conditions, developed 35·36 knots, or two knots more. Considering the reason for having destroyers at all, and how very desirable this high speed is for their particular purpose, it seems hardly open to question that people who are going to use torpedo-boat destroyers should do everything they can to encourage the development of this high speed; and that has been the case in nearly all the Navies in the world. It would be satisfactory, I have no doubt, to officers who have to command these classes of boats in time of war to know exactly what their speed really is; therefore, if some method of having a fair and equitable system of carrying out trial trips could be devised, it would be very greatly in favour of the efficiency of the class when used for warlike purposes.

The Hon. C. A. PARSONS, C.B., F.R.S., D.Sc. (Member of Council): My Lord and Gentlemen, to Sir John Thornycroft and his colleagues, and also to Mr. Yarrow, we are primarily indebted for the introduction of fast-running machinery in torpedo boats and destroyers, and I think we must now congratulate Messrs. Thornycroft on having achieved the highest speed under Admiralty conditions in their latest torpedo-boat destroyer. About four years ago the *Viper*, on a special trial, reached about the same speed, but the conditions then were less onerous, and the two performances cannot, therefore, be taken quite on all fours. The *Viper* burned coal, and she could not have maintained the speed for more than three hours at the outside. I think Mr. Thornycroft scarcely puts the case quite fairly in regard to the smaller class of vessels in concluding that 4,000 H.P. is the minimum limit for turbines. He does not state the case fully enough. He does not give us the radius of action of these coastal destroyers, nor the radius of action of similar boats with reciprocating engines. In the absence of this information it is very difficult to make any criticism or to argue the case. Assuming that a turbine-equipped coastal destroyer had a smaller radius of action, there are other advantages to be taken into account, which he justly attributes to turbines in the larger class of destroyers. Mr. Thornycroft quotes Messrs. Schichau's statement as to their boats being able to go to Colombo, a distance of 3,700 miles; but we surely have at home sufficient data to determine that question absolutely. Then, again, on page 67, as regards the question of cost, he says: "As soon as the change to turbines takes place, the price per horse-power goes up very considerably," and then he goes on to state that when we came to very large powers like there are in the 33 knot boats, the price falls. But surely some consideration must be given to the dates when these boats were built. On the whole, in fast vessels, the experience has been that turbine engines are cheaper than reciprocating engines, and this would appear to be very much so in the case of such very fast vessels as the 33 knot boats.

Mr. S. W. BARNABY (Member of Council): My Lord and Gentlemen, I think the success of this type of boat should be almost entirely attributed to Mr. Parsons. The amount of power which can be got out of the turbines has surprised us all, and none of us, I think, expected to have done quite so well as we have. As regards the criticism of Sir Philip Watts upon the author's statement of the comparative speed of the *Tartar* class and the River class, I hardly follow his argument, because the River class boats were also tried on the Maplins, and failed to get their speed there, and were then allowed to run on another mile. These new boats are more than ten knots faster than the River class on the Maplin mile. Two years ago, when Mr. Yarrow, junior, read his paper upon the relation of the depth of water and speed, we all recognised the importance of having a suitable mile. Mr. Yarrow, senior, proposed a resolution, which I seconded, asking the Admiralty to provide a deep-water mile somewhere, but an Admiralty officer who was present

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expressed the opinion that we ought to be satisfied with the Maplin mile, which was a very good one. So it is, for some boats. I myself, two years ago, published a curve calculated from Major Rota's experiments, showing the difference in the resistance of the *Tartar* class at different depths of water. From that one could easily see that one would expect a higher speed on the Maplins than in deep water. We recognised that at the time. A premium was offered for the speed of these vessels, but it only extended to an excess of one knot. The premium was for speeds up to 34 knots, but not beyond that, so we did not think it worth while to press the ship to much more than 35 knots, but we could have done so, and if the premium had gone on unlimitedly we should have been very pleased indeed.

As regards the comparison made in Mr. Thornycroft's paper between the different types of vessels favoured by foreign Governments, the question of depth of hull has been considered, as he says, by Messrs. Schichau as one of great importance. They like to make the depth of the boat small, so as to reduce its visibility. That may be a good thing for a torpedo boat, but hardly for a destroyer. For a destroyer you want a good gun platform, and the higher your deck is above the water, the better gun platform you get; and you also want to get a good all-round view. It seems to me that the seaworthiness resulting from increased freeboard is far more important than the question of visibility for boats of this type. The French type has been modified very much, as is seen from the author's diagrams. From the low freeboard of the original Normand type they have now come up to a freeboard very much like that of the British type.

We are all much distressed at the sad accident which has so recently happened to the destroyer *Tiger*, and we should like to express our sympathy to all those concerned in it, and to the Admiralty for the loss of such a good ship. We torpedo-boat builders, of course, naturally think seriously about accidents of this sort, even when they occur, as in this case, to vessels not of our own build, but one cannot see that anything could have been done to have saved that ship from such a mishap. When a vessel like a torpedo boat or a destroyer is designed, as it must be, to give just the required strength under the conditions which are laid down (which are seagoing conditions), it is impossible to provide a ship which can stand the blow of a vessel amidships sufficient to make anything like a large hole in the side.

The shape of the rudder is also an important question. I think Mr. Yarrow was the first to point out the advantage of having a very deep rudder. It prevents the boat from heeling when turning at full speed. The types which you see on the diagrams (Plates IV. and V.) are very diverse. When you go astern at full speed, the form of rudder which we have generally adopted, and which is shown here, has one disadvantage—that the water piles up at the stern, and you get a greatly increased area at the after part of the rudder, increasing the twisting moment, and causing greater difficulty of control. If it is considered necessary to control the rudder at the maximum speed which the boat is able to attain astern, there may be an advantage in such a form as M. Normand is now adopting, where no part of the surface of the rudder extends above the water-line. The results of the trials of this class of boat with turbines and oil fuel impressed me, I must say, very much. Having now been connected with the running of boats for so long, the experience of such a trial is very surprising to me. You work the vessel up to full speed in starting for a six-hours' run, after which you do not require to touch the turbines or the oil fuel apparatus. The speed goes on gradually increasing; every half-hour the speed is more than it was the previous half-hour; and, as far as one can see, it would go on steadily increasing as the boat lightens until the pumps begin to suck air instead of oil. It is a marvellous success, from the point of view of oil fuel as well as of engines, and I think the Admiralty are to be most heartily congratulated on that part of the success.

M. Augustin Normand (Associate-Member): My Lord and Gentlemen, I beg to thank Mr. Thornycroft for the part he has kindly attributed to the works of my firm in his interesting and valuable paper, and to present some complementary observations in connection with the platform adopted in all French destroyers up to the class now building. This arrangement, the first application of which we made in 1898 on the *Durandal*, had been proposed by my father as early as 1881. It is known in America under the name of "whaleback." Its aim is not only to remedy turtle-back forms, but also

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to improve, independently of those forms, the conditions of working and the sea-going qualities of the boats in heavy weather, and, moreover, to increase the height of the firing platform. On the destroyers of the *Durandal*, *Claymore*, and *Yatagan* classes the platform consists of wooden gratings placed about 2 ft. above the steel deck. The sea, passing freely between the deck and the platform, does not put any violent strain on the freeboard; only the spray reaches the service platform. If it happens that a wave breaks over it, it simply goes through the grating, and there is no fear of its sweeping over either men or superstructure in passing from one side to the other. The raised openings, being out of reach of the sea, may be utilised without inconvenience, especially for ventilation of engine-rooms. All the French destroyers up to the present day have been fitted with this platform, as well as a great number of foreign destroyers of the same type. The results have been uniformly favourable for all such ships, whether they have been used in the Baltic, Channel, Atlantic, Mediterranean, or in the Far East. A certain number of our destroyers have gone to the Far East, crossing the Indian Ocean without following the coast. They have, in numerous instances, accompanied the squadrons in heavy weather without being compelled to abandon their assigned position. In spite of the favourable results obtained with this arrangement, we have not kept it on our new types because of the high cost, of the additional weight, and, above all, because of the necessity in which we found ourselves of lowering it, to preserve the stability, when the armament was increased. In two destroyers now in our yard, the service deck will be about 1½ ft. lower than in the previous arrangement. It does not appear to us, however, that the older system ceases to have advantages for displacements over 400 tons. We ought to be grateful to Mr. J. E. Thornycroft for having brought together, in this interesting review, particulars of the destroyers built during the last few years. He was well qualified to make such a comparison, his firm having just delivered to the British Admiralty the fastest destroyer built up to the present time. It is, perhaps, a question whether the increase in displacement should be used for increasing speed only, or for increasing both speed and armament.

Sir JOHN THORNYCROFT, LL.D., F.R.S. (Vice-President): I would like to make one or two remarks on the question of speed with regard to the destroyer about to be delivered. In this particular case, a comparison has been made by Sir Philip Watts between the water in Stokes Bay and the shallow water in the Thames. I must beg to differ with Sir Philip in this sense, that the comparison is not one calculated to afford accurate information, because the water in Stokes Bay is not really deep water, when dealing with a vessel of that size. It is too deep to give the advantage of shallow water and it is too shallow to give the advantage of deep water; so that I think the suggested reduction in speed is a little excessive, and I would like to put the figure nearer to what the author of the paper has proposed. To me it is a novel idea that there should be any considerable advantage to be obtained from really deep water. I am not at all sure that even the Scotch measured mile at Skelmorlie is deep enough for speeds of something like 35 knots, because the configuration of the waves about a ship, and produced by it, depends more on the speed of the vessel than on its size.

Admiral Sir GERARD NOEL, K.C.M.G., K.C.B. (Associate): My Lord and Gentlemen, we have to thank Mr. Thornycroft for a most able paper, principally historical, bringing up to date all that has been done with regard to destroyers. Also, we have got out of Sir Philip Watts a most excellent reply. All that has been said, and nearly all that has been written, has been on the question of speed; but speed is not the only fighting necessity, and I am delighted to find that Sir Philip Watts thinks that they claim too much speed. I hope that armament will be considered when it is possible to reduce that speed from 35 to 33 knots. Speed is of little use to a destroyer if, when she has overtaken the enemy, she is not able to sink her. The armament at present is absolutely inadequate, but I understand from the Admiralty that they are going to increase it.

There is one other subject which I am very anxious about, and that is the use of oil fuel as being the *only* fuel on board these ships. Oil fuel is not obtainable in any possession of Great Britain, except to a very small extent. I think that is a matter for us all to consider most seriously. Moreover, when these ships go into action, and they attack half-a-dozen torpedo boats, or any other craft, they are practically

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unprotected. Coal gives us a certain amount of protection, but oil gives us none, and that is another matter for those in authority to consider.

Professor J. J. WELCH, M.Sc. (Member): My Lord and Gentlemen, there are one or two points I should like to touch upon. On page 60 Mr. Thornycroft has given the stresses which were allowed in the earlier vessels as being 9 tons in tension and 7½ tons in compression, and he mentions that in the later vessels the stresses are somewhat lower than that. I should be glad to know if Mr. Thornycroft thinks that in the later vessels we have gone rather too low in the matter of stresess. Personally, knowing the great experience which the Admiralty have, I am quite content to take their later value as representing the safest practice. I hope the remarks of Sir Philip Watts will not lead anyone to believe that the vessels of this class would not get their speeds in deep water. In the case of the *Cossack*, for example, which was built by Messrs. Cammell, Laird & Co., she was officially run on the deep-water Skelmorlie mile, and obtained well over the contract speed of 33 knots; whilst on the shallow-water mile—with different propellers, I may say, so that no direct comparison is possible—the speed was 34·7 knots. That vessel, we are glad to hear from Sir Philip Watts, has just been delivered, and is thus the first of the class to be taken over by the Admiralty.

Mr. J. E. THORNYCROFT (Member): My Lord and Gentlemen, I feel great difficulty in replying to some of the statements made. In fairness to ourselves as builders, I must venture to differ (not in a scientific way, but purely as a statement of results obtained) from the conclusions which Sir Philip Watts has deduced from the trials. I think many people would gather that the actual speed which was given in the paper as that of the vessel was not the speed which was obtained. But since that speed was obtained on a measured mile which has always been taken by the Admiralty as the standard mile in the past, and by the method of measuring the speed which has been recognised as the right one for a number of years, not only by the British Admiralty, but by others, one felt justified in giving that speed as the true speed of the vessel. If the recent knowledge which has been obtained with regard to the effect of the depth of water on the speed of different-sized ships affects the case, it is a matter to be looked at from the scientific rather than from an ordinary commercial point of view, or even, I think one may say, from a fighting point of view. When one speaks of a "shallow-water speed," I think one must qualify that by saying what the shallowness of the water actually is. The water at the Maplins is not really shallow, it is about 10 fathoms, and I think these boats may be expected to perform a great deal of their service in water of even less depth than 10 fathoms. The effect on varying speed and how soon the shallowness of the water affects a particular boat are questions about which our knowledge is quite in its infancy. If one takes the previous order of boats of the River class—boats of 600 tons and 25 to 26 knots speed—it is well known that this particular depth of 10 fathoms detracted from their speed. It was found that a better speed was obtained when in deep water. From experiments made, particularly by Mr. Yarrow, in running these boats in different depths of water, it was ascertained that there was a depth of water which was about as good for speed as deep water; but I do not think there are any actual trials on record which go to show that boats have got better speed in shallow water than in deep water.

Sir PHILIP WATTS: Yes, there are.

Mr. J. E. THORNYCROFT: I must bow to Sir Philip Watts, but I do not think we had those at our disposal. One must take into account the irregularities of bottom in tank experiments. How far Messrs. Denny were able to give the true effect of irregularities of bottom one cannot say. I believe they put obstructions at the bottom in the tank that had to be reproduced, but one cannot tell exactly what the bottom is like. I think it is possible there may be some advantage, but in the majority of cases, I think one would find that one could not do better than one would in deep water. The experience with some of these boats would seem to show that in the 40-fathom water of the Clyde one may get possibly a somewhat lower speed, but I do not think that Sir Philip Watts will say that it is more than a knot less.

Sir PHILIP WATTS: I do; it is more than a knot. We know approximately what it is.

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Mr. J. E. THORNYCROFT: Apparently Sir Philip knows, and later on, when these experiments are made comparing boats in shallow water and deep water, it is to be hoped that the results will be published, so that one may be able to utilise them in the future.

Sir PHILIP WATTS: Quite so.

Mr. J. E. THORNYCROFT: Admiral Sir Cyprian Bridge made reference to the method of making trials for this class of vessel, and stated that he considered it would be a desirable thing if an equitable method could be arranged for all the different builders. I am not sure that it is quite clear that the method by which all the boats are tried is the same, and that Sir Cyprian Bridge is under some misconception on this point. Each builder in the different parts of the country makes his trial on identically similar lines to the others, and, as a rule, with the same Admiralty officials, so that even the personal equation is reduced to a minimum, and there is only the question of one measured mile showing a different result from another.

With regard to Mr. Parsons' remarks about the relative economy at different speeds and powers, he objected to my statement that turbine machinery showed less economy at cruising speeds than reciprocating engines, and stated that it was not possible to argue against it without having actual comparative figures. I am not at liberty to give the radius of action of the latest British boats, and for this reason I referred to results obtained by certain foreign vessels. There is no doubt, however, that the radius of action with turbine-driven boats, at cruising speeds, is very considerably less than that of boats fitted with reciprocating machinery. The enormous range in power which occurs between full speed and cruising speed, the power for which is often not more than one-tenth of the power required for full speed, makes it very difficult to obtain satisfactory results.

Sir PHILIP WATTS: I do not know whether I may be allowed to say, with reference to a remark made by Mr. Thornycroft, that with regard to the Maplins being considered the standard Admiralty mile, Messrs. Thornycroft themselves discarded it in the case of the River class and went to Skelmorlie, and we agreed to their doing so. I think you will agree to that, Mr. Barnaby?

Mr. S. W. BARNABY: Certainly.

Mr. J. E. THORNYCROFT: That was simply because on the particular measured mile of the Maplins we were unable to get the speed with that particular boat, although it was more favourable for this faster boat.

Sir PHILIP WATTS: Quite so. In that particular case the correction was against them.

The PRESIDENT: Gentlemen, I ask you to join in a hearty vote of thanks to Mr. Thornycroft for his very interesting paper.

Mr. R. J. WALKER (Member) sent the following written contribution: I should like to endorse Sir Philip Watts' remarks as to the very interesting record Mr. Thornycroft has given of the advance made in the design, power, and speed of torpedo boats and destroyers during the last few years. The firm of Messrs. Thornycroft have undoubtedly played a very important part in the development and construction of this type of vessel, and Mr. Barnaby and the members of his staff are especially to be congratulated on their success in the excellent results attained in the *Tartar*. Reference has been made to the adoption of turbine machinery in this class of vessel, and I am afraid I must differ somewhat from the opinion expressed by Mr. Thornycroft on one or two points raised in his paper. As is well known, the *Viper* was the first destroyer to be fitted with turbine machinery. Mr. Parsons, in his remarks, has made reference to this vessel, and I would venture to refer to the results attained. The *Viper* was of the same dimensions and lines as the 30-knot destroyers fitted with reciprocating engines at the period at which the *Viper* was built. She had a heating surface of 15,000 sq. ft., as against the 13,000 sq. ft. in the 30-knoters, and on a three-hours' official trial, carrying the contract load stipulated by the Admiralty,

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she attained a speed of over 33 knots, and on a special run of one hour's duration a speed of 36·5 knots was reached. This was considered to have marked a distinct advance in the speed of destroyers. Since the days of the *Viper*, and as a natural result of the experience gained, considerable advance has been made in the arrangement and efficiency of turbine engines as applied to these types of vessels. Mr. Thornycroft appears to be in doubt as to the suitability of turbine machinery for torpedo boats of horse-powers below 3,000 to 4,000. As stated by Mr. Thornycroft, turbine engines are no doubt shown to greater advantage at the higher powers and speeds, but it may be interesting to cite that in 1904 turbine machinery of as low an I.H.P. as 1,800 was fitted to a torpedo boat, and a speed of 26·25 knots on a displacement of 100 tons was obtained. At 14 knots the coal consumption was found to be 2·2 lbs. per equivalent I.H.P. of a sister vessel with reciprocating engines, and after three years of continuous work the results obtained on trial have been shown to be fully maintained on service, whilst it is understood that very different results have been obtained from reciprocating engines of the same age, the coal consumption having largely increased from that obtained on trial. As regards the arrangement of machinery, overhauling, and care and maintenance in service, I cannot quite see that any serious difficulties exist, such as one is led to infer from Mr. Thornycroft's remarks in this respect. The engineer officers of turbine vessels in the British Navy are receiving the necessary training in the efficient running and adjustment of turbine machinery on service.

There is only one other point I wish to refer to, and that is in regard to Mr. Thornycroft's statement that at cruising speeds the efficiency is much less than that of reciprocating engines. It is not claimed that at powers which are low in proportion to full power the turbine is more economical than reciprocating engines, but from comparative results of turbine vessels with sister vessels fitted with reciprocating engines, it would appear generally that on actual service, and after two or three years of running, the turbine vessels at all speeds are at least as economical as the reciprocating engined vessels. Actual results are given in a paper* by Mr. Parsons and Mr. Ridsdale read before the Institution, at Bordeaux last year, of the coal consumption on service of turbine and reciprocating engines, from which it would appear that for recorded speeds as low as 7 knots the economy of the turbine vessel is equally as good as that of the reciprocating engined vessel. In conclusion, I would like to add that one of the most notable features observed in the running of turbine vessels is their sea-keeping qualities in heavy weather. It has been found possible to run the engines in almost any weather without the need of careful nursing, and it is only when the order comes from the bridge, due to considerations of safety for the vessel, that it is necessary to ease the engines. This, I think it will be agreed, is a very important factor in any war vessel.

* Trans. I.N.A., Vol. XLIX. p. 271.

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THE COMBINATION SYSTEM OF RECIPROCATING ENGINES AND STEAM TURBINES.

By the Hon. C. A. PARSONS, C.B., F.R.S., D.Sc., M.A., Member of Council ; and R. J. WALKER, Esq., Member.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 9, 1908 ; the Right Hon. the Earl of GLASGOW, K.C.M.G., LL.D., President, in the Chair.]

IN the early years of steam turbine design and development it became apparent that the turbine engine was capable of economically dealing with ratios of expansion far beyond the reach of any reciprocating engine whose limitations in this respect had been experimentally determined by many investigations.

In 1889 the first condensing turbine of about 100 H.P. was designed for an expansion ratio of 100 by volume, the expansion being effected in two turbines of the double parallel flow type, the low-pressure turbine (Fig. 1, Plate VI.) taking steam from the exhaust of the high-pressure at atmospheric pressure and expanding it down to 1 lb. absolute.

The striking feature presented by this design was the very high estimated efficiency of this low-pressure portion.

A separate low-pressure turbine was not, however, actually constructed till some years later.

In 1894 a patent was taken out for the " combination " of a reciprocating engine with a steam turbine whose object was " to increase the power obtainable by the expansion of the steam beyond the limits possible with reciprocating engines." The previous treatment of the steam is, of course, immaterial, provided that its condition of pressure and wetness on reaching the engine are known.

The first instance of a separate turbine worked from the exhaust of other turbines was in the *Turbinia's* machinery in 1897—the pressure at entry of her low-pressure turbine was about 9 lbs. absolute, and the exhaust 1 lb. absolute.

The slip ratio of her three shafts showed that the low-pressure turbine developed about one-third of the total horse-power obtained from the steam at 160 lbs. pressure, agreeing closely with calculations.

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In the year 1902 the combination of reciprocating engines exhausting into turbines was first put to a practical test in H.M. Destroyer *Velox*. In this vessel two small reciprocating engines were fitted for cruising purposes, of such power that, in combination with the main turbines, they would give an economical consumption at the speeds of 11 to 13 knots, the usual cruising speeds at the time the *Velox* was built.

The arrangement of machinery consisted of one main high-pressure and one low-pressure turbine on each side of the vessel, each driving a separate shaft, or four shafts in all. The two small reciprocating engines were coupled at the forward end of each of the low-pressure turbines. For speeds up to about 13 knots, steam was admitted to the two reciprocating engines, and expanded down to about atmospheric pressure; it then passed through the high-pressure, and thence through the low-pressure turbines to the condenser. This combination gave excellent results at these cruising speeds. For speeds above 13 knots, however, the reciprocating engines had to be cut out and steam admitted to the turbines alone.

With the advance of naval efficiency, the cruising speeds of war vessels have been increased, and in vessels subsequent to the *Velox* additional high-pressure turbines have been fitted, an arrangement which permits of good economy over a wide range of cruising speeds.

It may be said that perhaps the most important field for the combined system of machinery as applied to marine propulsion is for those installations where the designed full speed of the vessel falls below the range suitable for an all-turbine arrangement, the reciprocating engine working in the region of pressure drop where the conditions are best suited for it, and the turbine utilising that portion of the expansion diagram which the reciprocating engine is not able to utilise efficiently.

It is generally well known that an all-turbine arrangement has not been advocated by us for ships where the designed speed falls below 15 or 16 knots, excepting in some special cases such as yachts; and for vessels of moderate or slow speed, the combination system of machinery appears to be eminently suitable.

In a good quadruple reciprocating engine, the steam is expanded down to the pressure of release, about 10 lbs. absolute, and gains in economy as the vacuum is increased up to about 25 in. or 26 in., whereas, in a turbine, it is possible to deal economically with very low-pressure steam, and to expand this low-pressure steam to a low absolute pressure corresponding to the highest vacuum obtainable in turbine practice.

Figs. 2 and 3 on the following page illustrate the effect of vacuum upon steam consumption as the result of tests carried out on a reciprocating engine and steam turbine respectively, from which it will be noted that, whilst the curve for the reciprocating engine shows there is very little to be gained in economy with a vacuum higher than 25 in. to 26 in., the curve for the turbine continues to fall and economy to improve rapidly as the vacuum increases.

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Fig. 4 (Plate VI.) shows in diagrammatic form, the advantage of the combined system of machinery. The total area of the diagram represents the maximum energy that could be obtained, theoretically, from the steam, if it were expanded down to the pressure in the condenser. The area enclosed by lines A, B, C, D, and E,

FIG. 2.

EFFECT OF VACUUM UPON STEAM CONSUMPTION OF TRIPLE-EXPANSION RECIPROCATING ENGINE OF 200 I.H.P.

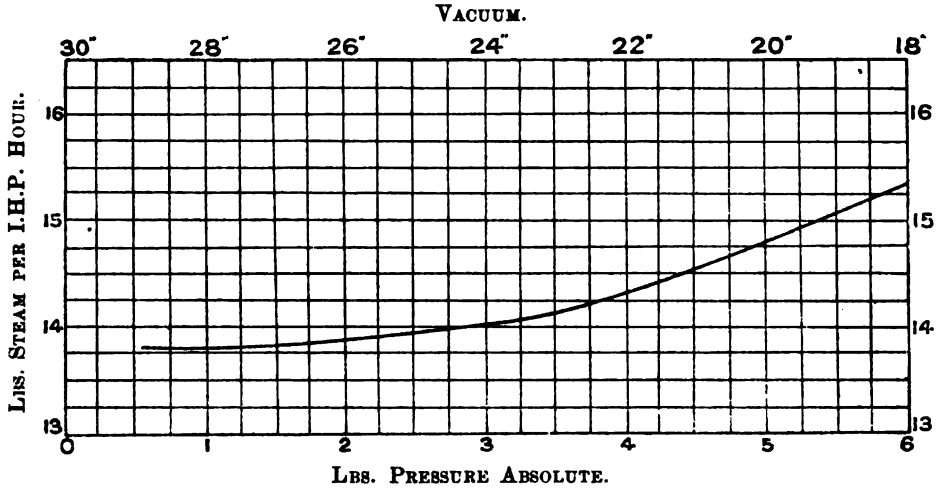
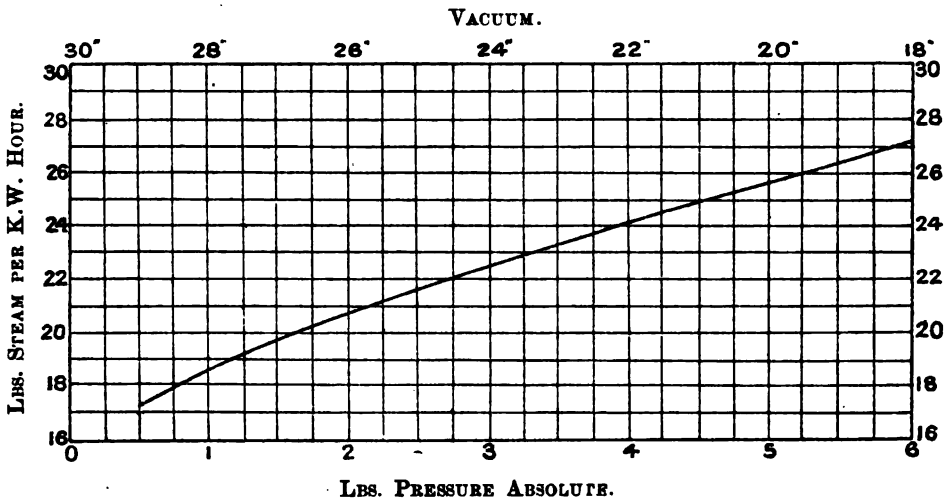


FIG. 3.

EFFECT OF VACUUM UPON STEAM CONSUMPTION OF STEAM TURBINE OF 1,000 K.W. TURBO-GENERATOR.



shows the theoretical maximum energy realisable in a quadruple engine from 200 lbs. pressure to 26 in. vacuum, and the area cross-hatched the additional energy that can be utilised in a turbine, but which cannot be economically used in a reciprocating engine.

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In a combination system, the most suitable initial pressure for the turbine, or the dividing line between the reciprocating engine and the turbine, will greatly depend upon the conditions of service of the particular vessel taken. The reciprocating engine, or engines, could be designed to exhaust at a pressure of between 8 lbs. and 16 lbs. absolute, or even at a slightly higher pressure, if necessary, to meet the conditions required. From an estimate of the theoretical efficiency under the various conditions of pressure as set forth in the following Table, it would appear, apart from any practical considerations, that there is nothing to choose between an initial pressure at the turbine of between 7 lbs. and 15 lbs. absolute, any pressure within this limit appearing to give the most economical result.

			Theoretical B.T.U. per lb. of steam.		
——	Initial pressure turbine.	Reciprocating engine back pressure.	R.E.	Turbine.	Total.
Assuming 200 lbs. absolute at Reciprocating Engine to 28 in. vacuum at condenser...	15	16	178	142	320
	12½	13½	189	131	320
	7	8	218	100	318

In the case of a vessel which runs on service continually at or about her designed full speed, an initial pressure of about 7 lbs. absolute at the turbine appears most suitable. In a vessel which does part of her running at the designed power, and part at a considerably reduced power, it is desirable to design the turbines so that the initial pressure would not fall below 7 lbs. absolute when running under the lower conditions of power.

It might be of interest at this stage to consider the disposition of the turbines in combination with reciprocating engines on board ship. The arrangement of the turbine, or turbines, depends greatly upon whether the vessel is to be fitted with single or twin-screw reciprocating engines. With a single reciprocating engine, one turbine, two turbines in "series," or two turbines in "parallel" could be fitted, each turbine driving a separate shaft in addition to the reciprocator shaft. With twin-screw reciprocating engines, an arrangement of one turbine in the centre of the vessel, two turbines in "parallel," or two turbines in "series," could be adopted. The arrangement which seems to commend itself generally to shipowners and builders, where twin-screw reciprocating engines are fitted, is the arrangement with the turbine on the centre shaft.

In 1901 and the two or three following years, alternative schemes were prepared from time to time. Fig. 5 (Plate VII.) shows an arrangement which was prepared in 1901 of a single reciprocating engine in combination with two low-pressure turbines in "series." The indicated horse-power of this proposal was 1,500, speed 11½ knots, and

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loaded displacement 5,300 tons. Fig. 6 shows another arrangement of one reciprocating engine, and a turbine on one side of the vessel. The I.H.P. of this vessel was 1,200 total, and the speed 10 knots.

The turbines in each proposal were designed for about 25 per cent. of the power, and the reciprocating engines the remainder, the turbines taking the steam from the reciprocating engines at 7 lbs. absolute pressure. It was estimated that the combination as applied to cargo vessels would be about 15 per cent. to 20 per cent. more economical than the ordinary triple-expansion engines usually fitted to this class of vessel.

Owing to the rapid development of the turbine industry for high-speed work, and the attention which was, in consequence, paid to this branch of the business generally, the development of the combination system fell more into the background than its merits and the wide scope of its application would seem to have deserved.

About two years ago, at the suggestion of Sir William White, designs were prepared of a combination system as applied to the intermediate type of liner of moderately large power and speed, and since that time numerous designs have been prepared for various types of vessels of speeds ranging from 13 to 16 knots.

By the courtesy of Messrs. Swan, Hunter & Wigham Richardson, who, for a considerable period, have taken much interest in this combination of machinery, the figures in the Table on page 82 are given of the comparative sizes of engines, power, &c., of the "combination" as compared with twin-quadruple engines, for a proposed steamer of 490 ft. in length, 13,600 tons displacement, 7,200 I.H.P., and 15½ knots speed.

In the combination proposals set forth in columns B and C in the following Table, it may be mentioned that in this particular inquiry the shipowners wished to have the advantage of the additional power and increase in speed of the vessel on the same coal consumption as for the twin-quadruple engines. In some instances an increase in speed might not be desired, in which case the boilers and engines could be reduced in size by the estimated amount of saving in consumption, so that the total indicated horse-power of the combination did not exceed that required with twin-quadruple engines. This would considerably reduce the total weight of machinery, and also the bunker capacity for a given distance. This saving in the weight of the machinery and in the bunkers would enable the vessel to carry an equivalent addition in deadweight cargo. Then, again, if we take the indicated horse-power of 8,300 for the combination, and assume that quadruple engines and boilers were required to give an equivalent power, the extra total weight of machinery would be, roughly, 160 tons, in addition to an increase of about 12 per cent. in coal consumption for the same power.

Fig. 7 (Plate VII.) shows an arrangement of twin-quadruple engines corresponding to particulars given in column A of the following Table. Fig. 8 (Plate VIII.) shows an

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arrangement of twin triple-expansion engines, working in conjunction with two low-pressure turbines, as set forth in column B, the reciprocating engine on each side exhausting into a turbine. By this arrangement an independent set of engines is

	A	B	C
	Twin Quadruple Reciprocating Engines.	3-Cylinder Triple-expansion Twin Reciprocating Engines, with two Low-pressure Turbines in parallel.	4-Cylinder Triple-expansion Twin Reciprocating Engines, with one Low-pressure Turbine.
Dimensions of Reciprocating Engines ..	$\frac{25,\ 36\frac{1}{2},\ 51\frac{1}{2},\ 75}{55}$	$\frac{27,\ 42,\ 66}{48}$	$\frac{26,\ 39,\ 46,\ 46}{42}$
Revolutions of Reciprocating Engines ..	84	85	100
Piston speed of Reciprocating Engines ..	770	680	700
Boiler pressure	213 lbs.	213 lbs.	213 lbs.
Estimated pressure at H.P. Receiver ..	200 lbs.	200 lbs.	200 lbs.
Initial pressure Turbines	—	7 lbs. absolute	7 lbs. absolute
Vacuum at Condenser	26 in.	28 in.	28 in.
Steam consumption, Main Engines only..	95,000 lbs. per hour	95,000 lbs. per hour	95,000 lbs. per hour
I.H.P. Reciprocating Engines	7,300	6,300	6,300
Estimated equivalent I.H.P. of Turbines..	—	2,000	2,000
Total I.H.P.	7,300	8,300	8,300
Per cent. increase power	—	13·7 per cent.	13·7 per cent.
Estimated speed	15·5 knots	16·2 knots	16·2 knots
Steam consumption, lbs. per total I.H.P. per hour Main Engines	13 lbs.	11·45 lbs	11·45 lbs.
Steaming weight of Engines and Boilers (Reciprocating Engines)	—	1,430 tons	1,455 tons
Weight of Turbine Installation	—	65 tons	70 tons
Total steaming weight	1,560 tons	1,495 tons	1,525 tons
Revolutions of Turbines	—	480	320

obtained on each side of the vessel. Fig. 9 shows an arrangement of four-crank triple-expansion engines and one low-pressure turbine, as set forth in column C.

In arrangements shown in Figs. 8 and 9 (Plate VIII.), for manœuvring in and out of

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port, suitable arrangements are made for changing the flow of steam of the L.P. cylinder exhaust of the reciprocating engine from the turbine to the condenser. This can be done in two or three ways. One method is to have an ordinary change valve of the piston type, or ordinary double-beat spring loaded valve actuated by links connected to the weigh shaft of the main engine, which would automatically change the flow of steam to the condenser when the engine was reversed. With this arrangement, when going ahead on one side of the ship, the steam from the reciprocating engine would flow through the turbine; but there does not appear to be any objection to this, even if we consider the twin-screw reciprocating arrangement with a single turbine on the centre shaft. It might be rather an advantage than otherwise, to allow the steam from the engine going ahead to pass through the turbine, as the centre propeller revolving would accelerate the feed of water on the rudder, and augment the turning power of the vessel. Figs. 10 and 11 (Plate VIII.) show such arrangements.

Another method would be to work these valves independently of the main engines, actuated by a hydraulic engine, or by an ordinary steam-driven reversing engine. With this arrangement, the low-pressure turbine would be cut out altogether and the reciprocating engine would exhaust to the condenser whether going ahead or astern during manœuvring.

Enough has perhaps been said as to the general arrangement and estimated econony obtainable in the combined system, and now it may be of interest to refer to the general application of the system.

The development of the combination is already rapidly taking place on land, where the exhaust steam from non-condensing engines, especially the winding engines at collieries, and rolling and mill engines, is being utilised in low-pressure condensing turbines. There are, at the present time, some 24 of these installations of the Parsons type delivered, working, and under construction, ranging from 125 kilowatts to 1,250 kilowatts, representing a total brake horse-power of about 17,000. In most cases the exhaust steam is supplied to the low-pressure turbine at 15 lbs. absolute pressure, and a vacuum of 28 in., and under such conditions about an equal amount of power can be obtained from the turbine as from the non-condensing reciprocating engine; thereby doubling the power of the plant without any further consumption of fuel. From several tests made with these exhaust turbines on land, a consumption of about 34 lbs. per K.W. hour can be obtained in a 500 K.W. machine with an initial pressure of 15 or 16 lbs. absolute to 28 in. vacuum.

In regard to marine installations, the combination is being fitted to a large vessel at present under construction at the works of Messrs. Harland & Wolff for the Montreal trade of the Dominion Line. The arrangement of machinery in this vessel is substantially as described in column C of the Table previously referred to, viz., twin four-crank triple-expansion engines exhausting into one low-pressure turbine driving the centre shaft.

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Messrs. W. Denny & Bros. are also at present building a vessel for the New Zealand Shipping Company, which is being fitted with the combination system of twin triple-expansion engines and one low-pressure turbine. This vessel is an exact repeat of two other vessels Messrs. Denny have built for the same owners, except as regards type of engines.

In addition to the above, the Turbinia Works, in conjunction with Messrs. A. Stephen & Sons, of Glasgow, are fitting the combination system to the yacht *Emerald*. In this vessel, which was one of the first to be fitted with an all-turbine arrangement, it is intended to make some modifications to the existing arrangement of machinery by introducing a reciprocating engine on the centre shaft in lieu of the high-pressure turbine at present in the vessel. This engine will be of the high-speed enclosed self-lubricating type, and is now being constructed by Messrs. J. S. White & Co. It is designed to exhaust into the two low-pressure turbines at about 15 lbs. absolute pressure, the dimensions of the engine being $\frac{12\frac{1}{2},\ 19,\ 30}{18}$ and revolutions about 350. It is expected that this vessel will be ready for trials in about four months' time.

DISCUSSION.

Sir WILLIAM WHITE, K.C.B., LL.D., D.Sc., F.R.S. (Honorary Vice-President), sent the following communication, which was read by the Secretary :—This paper has a two-fold value : it is a record of historic interest in regard to the conception and development of the idea of using low-pressure steam turbines combined with reciprocating engines in marine propulsion. It is also a most clear and comprehensive statement of the reasons why such a contribution will prove advantageous in ships of comparatively moderate or low speeds. In addition, it furnishes remarkable proof of the comprehensive view which Mr. Parsons took from a very early date of the possible arrangements which, in practice, might be made with steam turbines. My own information on the subject, in regard to what was contemplated by Mr. Parsons, goes back for many years ; and, in connection with the work done jointly by Mr. Parsons and his staff and Messrs. Swan, Hunter & Wigham Richardson, I had the pleasure of taking part. The problem first attacked was that of the cargo steamer, where there is, in my judgment, a great possibility of the future application of the combined system. In the course of the investigation, the point necessarily had to be considered at what stage in the increase of power and speed would it be found preferable to depart from the combination system and to adopt turbines only. This investigation naturally led to the conclusion that in many classes of steamships of the "intermediate" type fitted with considerable engine power, it would be desirable to adopt the "combination" plan. Subsequent investigations made for typical ships confirmed the possibilities of economy in coal consumption with the alternative advantages either of increased speed with a given coal consumption or of greater dead-weight cargo-carrying capacity for a given speed. In dealing with these problems, the introduction of electric energy at some stage was considered, in order to enable the high rotary speed essential to turbine efficiency to be utilised in association with the low speed of revolution of the propeller shafts.

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Other methods of transmitting or converting energy were investigated, and other workers in this field have no doubt been proceeding independently, and some of their results have been published. So long ago as 1902, Colonel Soliani proposed an arrangement for associating reciprocating engines with two turbines in triple-screw ships. Mr. Yarrow applied to a torpedo boat destroyer a similar arrangement, and this was described in Professor Rateau's paper read before the Institution. In such cases, however, the primary object is to deal with and secure economy of coal at low cruising speeds in certain types of warships. At the present time the suggestion is again being put forward to use electric motors for driving propeller shafts direct, and in the generation of the necessary electric energy steam turbines may, of course, well find a place. In all probability, out of these various investigations and experiments there will emerge some practical advances in steamship propulsion, tending to promote economy in vessels of moderate or low speeds where the use of steam prime movers is continued. It is, of course, possible that the development of internal combustion engines will prove a formidable rival; but the highest authorities on that department of engineering are clearly of opinion that before gas engines of large dimensions and great collective power can be successfully installed on board ship many different problems will have to be solved; and in the interval the combination of systems described in this paper is such as can be immediately applied with beneficial results. Experiments already arranged for in certain ships now building will furnish conclusive proof, at an early date, of what may be hoped for in this direction, and I am sanguine that the start thus made will lead to extended use of the combination system in some form.

Mr. JOHN WARD (Member of Council): My Lord and Gentlemen, as representing my firm, I may say that we were, as you know, early associated with Mr. Parsons in the introduction and development of the turbine in the Mercantile Marine, and our respect for it and its inventor has gone on strengthening and deepening with time. Naturally, when Mr. Parsons brought before the professional world his latest proposal of combination machinery (reciprocating and turbine), he found us easy converts, and as such we, in turn, have tried successfully to convert some of our clients. One of these, as Mr. Parsons has stated, is the New Zealand Shipping Company, for whom we have built a considerable number of steamers, including two of similar size and form to the one now being fitted with this combination of machinery. I may mention that the dimensions of the vessel are 465 ft. by 60 ft. The speed is a moderate one of about 12 knots loaded, while the distance to be run, out and home, practically represents a voyage round the world. In the faith held by Mr. Parsons and others, as to the realisation of the economy claimed for the principle, we fully agree. We also remember that in this as in other matters "faith without works is dead," and for that reason we refrain from stating meantime any details, until the principle has been tested and tried on its merits, against the similar ships owned by the company and running on the same service, but fitted with reciprocating engines. We hope that at our next meeting Mr. Parsons may be able to read a supplementary paper on the subject, in which the results will show that his faith has been justified by works. The saying of my namesake, Artemus Ward, that you should "never prophesy unless you know," is worthy of remembrance, and so, beyond expressing my belief in the success of the principle, and that it will fulfil in every particular the expectations as to economy and general utility claimed for it by Mr. Parsons, I will not at present go. We are making a considerable number of experiments in our tank in order to arrive at the best combination of propellers and machinery, and thus ensure as far as possible a satisfactory solution of the problem.

Mr. MARK ROBINSON (Member): My Lord and Gentlemen, in this very important paper Mr. Parsons, in his desire to combine with his turbines only the very best type of reciprocating engines, has confined his recommendation to triple and quadruple expansion engines. I want to ask him whether the case for the combination is not even stronger if turbines and *compound engines* be used together. Mr. Parsons tells us that it makes very little difference whether the initial pressure in an exhaust turbine is 7 lbs. or 15 lbs., and he says it may be higher. In this I entirely agree, as I also do with his remarks about the inutility of high vacua in reciprocating engines. Years ago, high-speed engine makers came to the conclusion that about 26 in. of vacuum is really the highest that could be used economically in a reciprocating engine; that

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is to say, if you take into account not only the thermal efficiency, but the *brake efficiency*, and perhaps, I should add, the cost-of-plant efficiency as well. I need not say that all reciprocating engine builders have lamented their inability to make profitable use of a really high vacuum, due to the fact that an attempt to carry expansion to the extent which, in a turbine, is both profitable and easy would involve the use of a low-pressure cylinder too big to go into the ship. Thanks to Mr. Parsons, we now realise that the turbine is not only a magnificent engine itself, but is also a great help to the reciprocating engine, for it can be made to serve as an ideal low-pressure cylinder, in which expansion can be carried to any extent in a very small space.

In times past, my firm made a great number of reciprocating engines exhausting into the atmosphere, under strict guarantees of economy, and many trials and much study left it an open question whether it was really worth while, even with 200 lbs. pressure, to make those engines triple expansion. The total range of temperature was but doubtfully sufficient to justify three stages of expansion, and if the exhaust pressure of the engine be raised to even a little above atmospheric pressure —say to 20 lbs. absolute—it is certain that the brake efficiency, and perhaps even the "indicated" efficiency, of the engine will be as good compound as triple. In some classes of steamers, where the number of cranks has not to be studied from the point of view of vibration, the most economical arrangement, and, therefore, from the owners' point of view, the most efficient arrangement, would be to have a two-crank compound engine exhausting, at a few pounds above the atmosphere, into a turbine or turbines. This need not involve materially longer turbines, for the size and cost of a turbine are practically settled by its low-pressure end; it is the final volume of the steam as it passes to the condenser which settles the greatest diameter of the casing and the length of the blades. It is only with the object of making still stronger the very strong case which Mr. Parsons makes out for this most valuable combination, by showing that the field for its employment is even greater than his paper suggests, that I have ventured to say these few words.

Mr. Reginald Morcom (Visitor): My Lord and Gentlemen, after the many controversies of the past, the title of this paper seems like a very burying of the hatchet. Certainly, the scheme of taking full advantages of the merits of either turbine or reciprocators, or both, is a desirable one, but I hope I may be excused if I say that I am disappointed in the paper. It seems to be made up of a little ancient history, a few descriptions of some of the many possible arrangements on ship-board, accompanied by drawings of the valuable character that one associates with patent specifications, a résumé of contracts executed by certain firms, a brief reference to land practice which is remarkably like something I have read before, and a very short analysis from a thermodynamic standpoint. Not being a naval architect, I will not criticise that part of the paper which is purely descriptive, and before this Institution further remarks on mere land practice would be out of place; but I hope a few words on the short analysis will be to the point. The data given in the paper are incomplete. Firstly, a comparison is made between a 200 I.H.P. reciprocator and a 1,000 K.W. turbine. A large reciprocating engine shows a higher thermodynamic efficiency at the low pressure end than a large turbine, and a more equitable pair of curves ought certainly to be available. There are many types of reciprocating engines and of turbines, and the efficiency, and back pressure curves vary with the type of engine, but no type is here specified. The behaviour of engines with regard to varying vacua is affected by initial pressure and super-heat, but no figure is given for either of these points. Such additional information would make the curves given more useful. The table on page 80, in dividing up the heat units between turbine and engine, makes no reference to the thermal efficiencies of the two machines, although this is the crux of the problem. The turbine is a comparatively inefficient machine with any back pressure, the reciprocating engine is a fairly efficient machine with considerable back pressure, but its efficiency deteriorates as the back pressure falls, till at about 1 lb. absolute it is as inefficient as the turbine. I have for several years been closely following up this subject in experiment and investigation of results, and have collected some valuable data on this point of the relative thermal efficiencies; but, following the example of several speakers on turbines lately, I will keep them up my sleeve.

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Professor R. L. WEIGHTON (Member): My Lord and Gentlemen, I rise chiefly for the sake of asking a question or two. In bringing such a paper as this before the Institution, the authors are bringing forward a subject in which all marine engineers are much interested at the present moment. Those of us who have not had the opportunity afforded us of investigation under rather peculiar circumstances—namely, under this low initial pressure which is proposed for turbines—are very anxious to get to the bottom of the economical performance of the combination. It seems to me that the success of such a proposed combination reduces itself to economy pure and simple, and it is from the point of view of economy that I want to call attention to a few points. The first point that occurs to me is, that I cannot make much of the diagrams on page 79. I do not wish to labour this matter, because I know the authors of the paper merely give these diagrams as introductory to the main parts of their paper; but still we do expect comparisons to be valid. Diagram Fig. 2 gives the economy of an engine in terms of indicated horse-power at variable vacua. Diagram Fig. 3 gives the economy in terms of useful horse-power—*i.e.*, per K.W. hour. We all know that the difference between these two methods of presenting results is essential and radical. Indeed if the first diagram were converted from indicated horse-power to effective horse-power, the curve would probably be of quite a different character altogether. Incidentally, I ought to mention that there is a discrepancy here, which may possibly be due to a printer's error. In the last paragraph on page 78, the authors say*: "From which it will be noted that whilst the curve for the reciprocating engine gives the minimum consumption at between 25 in. and 26 in. vacuum." It will be noted that in the diagram to which that refers there is no minimum consumption. If Diagram 2 be changed so as to give the consumption in terms of useful horse-power, it will be found that there is a definite minimum consumption, but as to whether that minimum consumption will occur at 25 in. or 26 in. vacuum or not depends entirely upon the proportions of the engine and on the conditions under which the engine is working. It depends largely upon the ratio of the cylinders. If you have a very large ratio of cylinders from the last to the first in a reciprocating engine you will have the minimum consumption at a higher vacuum than if the ratio is small. Then there is a second point with regard to these diagrams, and, indeed, with regard to the whole of the comparisons which are given in the paper; and it is this. Have the writers included in these figures with regard to consumption per horse-power thermal units or merely pounds of water independent of hot well temperature? That will make a considerable difference.

Again, in the table on page 82, we have a comparison between the reciprocating engines working with a vacuum of 26 in., and the combined system working with a vacuum of 28 in. The difference in thermal units expended to obtain the higher vacuum is so considerable that it will make a good deal of difference in the comparative results. I have not been able to gather from this paper that the thermal unit aspect of the question has been considered. Then there is another point on which I feel that the authors' paper would be doing their scheme a service if they gave us more information. Referring to the table on page 82, the steam consumption of main engines is given in the gross as 95,000 lbs. per hour. Would the authors oblige us by dividing that up between the turbine working at the pressure they propose and the reciprocating engine? All of us who have been accustomed to deal with reciprocating engines could, if we were given information as regards the turbine, make our own allowance as regards what would be the loss incurred by the reciprocating engine exhausting into the turbine. That is a point as to which I think we could do without the assistance of experiments from anyone. And here it appears to me that the idea of Mr. Mark Robinson is a sound one, in so far as if the authors of this paper contemplate using a materially higher initial pressure than 7 lbs. in their turbine in the combination, it is very probable that the double expansion engine would give a better result than the triple, and the quadruple would, of course, be out of the question.

Mr. A. E. SEATON (Member of Council): My Lord and Gentlemen, I only wish to say that it is only doing justice to the memory of a very honourable man, whose name has often been mentioned within these walls, to call attention to the fact that sixty-two years ago, before most of us were born,

* This paragraph has been amended by the author.—ED.

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Henry Bessemer took out a patent for a low-pressure turbine as an adjunct to a reciprocating engine. It was a reaction turbine, and, of course, as might be supposed, of a somewhat elementary and simple kind; but, strange to say, he suggested that it might be put with advantage in the condenser. Our friend Mr. Parsons has done his best, I think, to put practically his L.P. turbines into the condenser, so that he is, in both respects, really following the footsteps of a man with a very worthy and honoured name. The only other point, perhaps, I might touch upon is that experiments which have been carried on by my friends, Mr. Reginald Morcom and Mr. A. Jude, confirm the surmises of Professor Weighton and others, namely, that there is evidently more economy to be got with a low-pressure turbine at the end of a compound rather than a triple or quadruple expansion engine.

Mr. J. M. Allan (Member): My Lord and Gentlemen, there is one aspect of this paper upon which I would like to ask for a little information. While I take it for granted that the energy that Mr. Parsons says can be obtained is correct, I am not quite sure about the utilisation of it, seeing that the proposals under discussion are primarily for application to ships of moderate speed. In this connection the question of the shape of the ships is, I think, a very important one. For instance, in slow-speed cargo boats we may take it for granted that large propellers running slowly are the best. Then, with regard to applying the combination to these slow or moderate-speed steamers, I should like to know, if Mr. Parsons or Mr. Walker would tell us, what they would advise as to the machinery arrangement in the ships. If we are going to have a slow-running screw, it would be better, it seems to me, to have it on the centre shaft, and to arrange for the two quick-running turbines to be upon the wing shafts. It is upon this latter point that I would like to know if these gentlemen could give us some information. At present they are dealing with their land experience, and that is altogether unhampered by this other question.

Mr. Thomas Bell (Member of Council): My Lord and Gentlemen, I thought, or at least I hoped, that some of the speakers would refer to the great service Mr. Parsons has rendered to the old reciprocating engine. He has rendered as great a service, in my humble opinon, as has been done both by the application of compounding and then tripling and then quadrupling. In the original compound engine (I will not be certain as to the exact degrees, for I have not the book with me) there was a range in the cylinders of about 100 degrees in the high pressure, and, say, 120 or 130, according to the vacuum, in the low pressure. The triple-expansion system was then applied to engines, and the range in the high pressure was brought down to about 60—in the intermediate, say, to 60 or 70—but the low pressure was still left with about 120 degrees. Quadrupling, again, reduced the range of temperature in the high pressure, first intermediate, and second intermediate, but the poor old low pressure was left, as before, with a range of from about 110 to 130. Mr. Parsons, by the application of the turbine, at last comes to the aid of the low pressure, and the range can be reduced by from 40 to 60 degrees, which must have a very marked effect on the economy. I am not a deep student in this matter, but perhaps Professor Weighton, and others who have given the matter great study, could let us know what improvement would be made by this reduction in the range of temperature. It appeals to our own common sense that it must be a very great one indeed, and this is over and above all the gain Mr. Parsons gets below the 10 lbs. pressure absolute.

Mr. R. R. Bevis (Member of Council): My Lord and Gentlemen, I have listened to this paper with a very great deal of interest, because, some years ago, I got out two or three designs for some friends, utilising this combination system, based on triple-expansion engines for the wing shafts and a large centre turbine. Unfortunately, the matter fell through, and we were unable to obtain an order for such an instalment. Latterly—and possibly Mr. Parsons might give us some further information on the point—my attention has been directed by investigation to the point where the desirability of using an all-turbine arrangement ceases. I have formed the opinon that, as far as possible, consistent with speed conditions, we should have all-turbines in preference to any combined system. The difficult point to arrive

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at is where exactly to draw the dividing line. I see two difficulties in the combination system; one of them is the fact that you will be using in your turbine oily or dirty steam, and it might interest members to know how it is proposed to deal with this matter by those who are now practically carrying out this system. The other objection seems to me to be that it involves a considerable amount of extra weight—possibly, what you might term waste weight—inasmuch as the reciprocating machinery will have to be made very much heavier than usual to meet Board of Trade requirements, due to the fact that at certain stages you may have the full boiler pressure, and, of course, the full vacuum when reversing.

We have lately investigated the best type of reciprocating engine to use in such a combined system, and we have, in common with the views expressed by certain other gentlemen, who have evidently considered the matter, come to a similar conclusion, namely, that possibly the best form of reciprocating engine for the purpose is the three-crank compound, with an initial steam pressure at the turbine such as will give, at any rate, an approximation to equal distribution of power through the shafts. There seems to be no serious objection to maintaining slightly higher pressures at the turbine than those indicated by the writers of the paper. Whether the casing is made to stand 15 to 20 lbs. pressure, or merely atmospheric pressure, the thickness of metal would be practically the same, and I do not think the economy of the combination would be appreciably affected either way within these limits of pressure.

Mr. W. J. Harding (Visitor): My Lord and Gentlemen, many of us have had opportunities of studying the results given by the turbine system during some years, and have appreciated the efforts of the author, who has for so long devoted his brains and energy to improving the science of steam engineering; and we all rejoice at his success. We should be very glad if he would give us some of his experience where failures resulted; for failure is the great teacher, and the stock in trade of all seekers after knowledge. I think it is great heresy to say of the reciprocating engine, that any sea-going engineer would be content with a vacuum of 25 or 26 in., if he could get the pointer of the vacuum gauge right up to the stop. All should agree that the higher the vacuum obtained, the greater the economy, for then the coal shovels would not have to do so much work. History, including patent records, tells us that the quest for the perfect rotary engine has long occupied busy brains. Hero, the Egyptian, tabulated results; a thousand years later we have records of mechanical steam toys. The Jesuits of the seventeenth century gave much thought to the subject, and actually produced large working models of steam road cars and paddle-wheel vessels, and gave a unique exhibition before the Emperor of China, at Pekin, in 1642. These were operated by water-tube boilers. We have in our records about 2,700 patents for rotary engines, including many proposals for combinations with reciprocating and other engines, modes of working, construction of details, and other items. Mr. Seaton has mentioned Bessemer's proposal to place the rotary engine within the condenser, and there were other proposals as combinations, one of the earliest being to lead the exhaust steam from a reciprocating engine through its own fly-wheel, the latter then acting as a rotary engine of the "Barker's Mill" type. Other schemes were for the fly-wheel to revolve inside a case, or in a condenser. Others again were for using the rotary engine as the high-pressure engine, with the reciprocator as the low-pressure engine, and *vice-versâ*; while others were for combining rotary engines of different types for multi-expansion.

Some figures have been published showing the duty given by steam of given weight in reciprocating and turbine engines. The great economy given by the turbine when using low-pressure steam makes one regret that this knowledge was not promulgated earlier, seeing that the national quest for economy prompted the adoption of pressures up to 300 lbs. per square inch, and much sorrow ensued; and it is now generally acknowledged that no advantage is gained by using steam of more than 180 lbs. pressure in any steam engine, provided sufficient cylinder capacity be provided, and efficient well-insulated lagging be fitted. The Parsons turbine shows that, by using steam of about 10 lbs. initial pressure 4·177 horse-power may be obtained by the consump-

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tion of 100 lbs. steam in weight per hour. An ordinary reciprocating marine engine will not give more than 2·4 horse-power when using the same weight of steam per hour in one cylinder, at the same initial pressure. The average of twenty-eight prolonged experiments with marine engines of the triple-expansion type at various grades of steaming shows that 100 lbs. of steam per hour will give 6·21 horse-power, an average of 2·07 horse-power in each cylinder. From this it is apparent that the combination proposed in the paper will result in 8·32 horse-power being obtained, using the same 100 lbs. in weight of steam per hour, viz., 2·07 horse-power in each of the high and intermediate cylinders. and 4·177 in the low-pressure turbine. Thus, if there be any merit in careful water-consumption experiments, the proposal in the paper will give an increase of power of over 33 per cent. for the same weight of steam as used in a triple-expansion reciprocator. One welcomes the proposal in the latter part of the paper, viz., that the reciprocator, in combination, shall be of the closed pump-lubrication type. This will certainly be a rival to the turbine in that the watching of the latter, while at full speed, is so distinctly comfortable, as compared with the watching and feeling of the multitude of working parts all in sight.

Of the 2,700 patents for rotary engines about 20 per cent. are for turbines, this percentage increasing, and showing the survival of the fittest. I take it that many patents bearing the same honoured name of our greatest turbine inventor show that investigations giving increased experience are still progressing, so much so that one may be inclined to foretell that the turbine of five years hence will be very different from the present type. I should like to express the opinion that, if the makers of high-speed reciprocating engines were to borrow from the experience of the author, and largely increase the sizes of eduction and exhaust pipes, economy might ensue, for experience has shown that by decreasing back-pressure, especially between the L.P. cylinder and condenser, increased power has been given for the same weight of steam used.

Major Ferretti, R.I.N. (Member), sent the following written contribution to the discussion :—The property of the turbine to utilise that portion of the energy obtainable from the steam, which is not realisable in any triple or quadruple expansion reciprocating engine, is an undeniable fact, and the combination of reciprocating engines exhausting into turbines cannot fail to prove, in theory as well as in practice, more efficient than any all-piston or any all-turbine machinery installation for propelling purposes, under the present screw propeller's conditions, for vessels of moderate or slow speed. But I can scarcely regard this "combination system" as a real step in advance towards the development of the marine steam turbine, because I see in it rather the peculiar character of a temporary compromise than that of a definite settlement of the question of the most suitable propelling machinery for vessels of moderate velocity. It might be, however, of some interest to consider why the all-turbine arrangement has not, up till now, been advisable for such vessels—that is to say, for the majority of the vessels—and how to remove the obstacles which prevent it from becoming of general application. All the troubles arise, in my opinion, from the necessity to meet the requirements of the screw propeller. "I believe that we are on the eve of a great revolution in marine propulsion," lately remarked Mr. W. B. Stearns before the Society of Naval Architects and Marine Engineers of New York, alluding to the advent of the internal combustion motor. Personally, I believe that we are already in touch with it, ever since the advent of the steam turbine, and that one of its first results will very likely be to do away with the old screw propeller in favour of a more suitable device, to be connected without inconvenience to the high-speed non-reversing engines which may be expected to propel all sorts of vessels in the future. I do not think it necessary to spend much time or to put forward many figures in order to demonstrate the unfitness of the screw propeller to be coupled with any engine of high angular speed, as, for instance, the steam turbine, because it is well known how conflicting their requirements are, and how much the efficiency of each is more or less reduced in all the turbine-driven craft at the present time. When this difficulty came to be realised, attempts were made to get rid of this trouble by making the screw propeller move through the medium of some suitable gears or electric appliances. I do not, however, believe this solution of the problem to be the most advantageous, especially in the case of powerful vessels, where the complexity of

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the machinery is so great that it does not appear possible to introduce complications without giving rise, perhaps, to worse troubles than those that are to be cured. In any case, it seems scarcely advisable, from a practical point of view, to introduce an arrangement involving discontinuity between propeller and prime mover, as the security of vessels and the reliability of engines depend, first, on the fact that engine and propeller move together at the ends of the same shaft, without any sort of intermediate links.

In my opinion, rather than pursue the attempt to reconcile what seems to be incompatible, it would be more profitable to look for a more suitable device to take the place of what henceforth will be the obsolete screw propeller. That propeller will prove the fittest which does not in any way reduce the efficiency of its prime mover, or necessitate any increase of dimensions, weight, or cost. That is to say, the ideal propeller for turbine steamers should allow the number of revolutions to be so high as to make the marine steam turbine as efficient, as compact, as light, as simple, as the land steam turbine has long ago proved to be. It is not necessary to recall here how the steam turbine, to make a really profitable marine prime mover, requires not only great peripheral velocity, but, as far as possible, facility to rotate at a constant speed in the same direction, and how often, on the contrary, the propeller in its present form has to be driven in either direction at a variable velocity. This is the actual origin of that sort of incompatibility I was referring to, and this is the point which deserves the most careful attention. Many devices, of course, may be designed to put an end to the defective arrangement existing in the present turbine steamers, and to take advantage of all the good that the high-speed motors can do to marine propulsion. It is essential to go carefully into the question, and not to leave it unsolved for lack of proper and exhaustive trials. Nobody, of course, can reasonably be expected to carry out such expensive experiments alone, but it is highly desirable that everybody who is acquainted with the subject should publicly state his views, and provoke, by all means in his power, a general and open discussion, from which light could not fail to be thrown on the subject.

I venture to suggest that a convenient solution of the problem would be found by getting back to the so-called "reactive propulsion" which has so often attracted the attention of naval experts, by its unsurpassed simplicity and soundness of principle. As everybody knows, practical experiments, not totally deprived of satisfactory results, were made at different times and in different countries with vessels fitted with "water-jet propellers," compared with screw vessels of the same size and power, and that the final result proved anything but discouraging is shown by the tremendous number of patents granted at various times for inventions relating to "reactive propulsion." The greatest difficulty to overcome with the ordinary piston engine was to arrange for such a pumping plant as was necessary to produce, to conduct, and to discharge with high efficiency the powerful streams of liquid intended to propel the vessel ahead and astern. With the increase in the size and speed of ships, the said arrangement grew more and more difficult, so that the reaction system of propulsion seemed already doomed when the steam turbine made its first appearance in the field of naval engineering. Now, as any high-speed rotary engine is admittedly as fit to operate a pumping plant for propelling purposes as the slow reciprocating engine proved inadequate, it does not appear quite unreasonable to go back to the old-fashioned "jets of water" whose ejection causes the reaction which propels the vessel. As on land a great number of stationary turbo-pumps of various powers have for so long been in successful operation against any head, for all purposes, with high efficiency, low cost, and small size and weight, I cannot see why they should not give on board ship the same satisfactory results for propelling purposes. They might be eventually installed in such a number as to comply with any requirements regarding size, speed, manœuvring qualities, and security of the vessel. They could be placed so close to the prime movers as to eliminate practically all the heavy and bulky shafts of the present steamers, and be so arranged outside the sides or the bottom of the vessel as to avoid any intricate eddying of water currents in the interior of the hull. And, last but not least, might they not be so designed as to be directly operated from the bridge, bringing the propelling mechanism under the very control of the steersman ?

Whatever system of propulsion may be found most suitable to comply with the requirements of turbine or motor steamers, it is an undeniable fact that the security of the vessels would be greatly increased by providing them with the means of being handled from the bridge, so that they could be stopped, started

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at any speed ahead or astern, reversed, or checked, the engines meanwhile rotating at a constant speed in the same direction, thus rendering it unnecessary to give notice to the engine-room for any change of speed or reversal. These are the right lines on which, in my opinion, the problem in question ought to be dealt with in order to get rid of the troubles of the present arrangement. Any naval engineer, in his first experience of the present turbine steamers, cannot help feeling rather surprised and perhaps a little disappointed at finding himself in presence of the heavy, bulky, and slowly rotating turbines, which are only a little less complicated than reciprocating engines, and observing the vibration that visibly shakes the whole of the ship, more especially if it happens that he is well acquainted with the quiet, steady running of turbines for electrical purposes. He cannot help, perhaps, establishing in his mind a comparison which would prove anything but favourable to the present marine steam turbine, and if he likes to go into detailed figures he will be somewhat astonished to find, for instance, how much coal could be spared had it been found practicable to join the most efficient screw propeller with the most economical steam turbine on board the numerous turbine vessels already in existence. I do not expect, of course, the "reactive propulsion" which I advocate to give at once the excellent results in practice which, from careful but only theoretical investigations, may be anticipated. It will have mechanical difficulties and obstacles to overcome, errors to rectify, as with everything that is new and untested; but, at any rate, I consider it as well fitted to render the all-turbine arrangement of general and successful application for all sorts of vessels, whatever their power and the speed.

Colonel Nabor Soliani (Member) sent the following written contribution:—If I have understood the paper rightly, the main object of the "combination" is to improve the efficiency of reciprocating engines in those cases where the full speed of the vessel is too low to be suited to an "all turbine installation." A full speed of 15 to 16 knots is given in the paper as the limit below which the province of the "combination system" may usefully commence, and, nowadays, war vessels of all kinds have, with very few exceptions, speeds above that limit. It may be thought, therefore, a natural inference that for those vessels the utility of the combination is excluded, and an "all turbine" arrangement more convenient. Perhaps this inference is not absolute, and need not be strictly adhered to. It seems, to me at least, that war vessels need not be excluded from the possibility of a useful "combination" arrangement of engines. For, although their full speed is high, they generally steam at cruising speeds for which the "all turbine arrangement" is not so well suited. This difficulty has been met, it is true, by the addition of cruising turbines; but, although these turbines, as it is stated in the paper, permit of good economy over a wide range of cruising speeds, it is not certain that in all cases the range is wide enough—as wide, at least, as would be convenient and possible with reciprocating engines. Cruising turbines, moreover, which are of no use, and work idle at high speeds, require for their installation weight and space which are not trifling. The combination system would do away with this necessity. An "all turbine arrangement" requires also special turbines for reversing, and this is an unwelcome adjunct, which would, by the combination system, be dispensed with; perhaps with the additional important advantage of more prompt and efficient backing and manœuvring power. Of course, the "combination system" best suited for warships would be somewhat different in details and proportions from that described in the paper as convenient for merchant vessels, on account of the different conditions of service prevailing in war vessels. In these cases the turbine portion of the combination, in lieu of being a mere improving final stage to the reciprocating engine, ought to be, in my opinion, a more substantial part of the system, both in function and in proportion, the reciprocating engines being only of sufficient power to ensure good backing and manœuvring at all speeds. Instead of being fully three-quarters, as in the case of mercantile vessels, the power of the reciprocating engines in war vessels would probably be, at full speed, only 30 to 40 per cent. of the total combined power, leaving, therefore, a far greater share to the turbines. Moreover, the exhaust pressure from the reciprocating engines, and consequently the initial pressure in the turbines, increasing with increasing speeds, would be great at full speeds. Therefore the turbine portion of the system ought to be arranged so as to meet these varying conditions. In support of the possible convenience, in some

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form, of the "combined system" in war vessels, the successful torpedo boat *Goyaz*, with "combined machinery," built recently by Messrs. Yarrow & Co., for the Brazilian Government, may be referred to. With my limited knowledge of the practical operation of turbines, it would be presumptuous to go further, and I ask the kind indulgence of the authors of the paper in their consideration of the above remarks.

The Hon. C. A. PARSONS, C.B., M.A., F.R.C.S., D.Sc. (Member of Council): My Lord and Gentlemen, several speakers, particularly Mr. Mark Robinson, have referred to the line of demarcation between the reciprocating engine and the turbine. This will depend, as far as can be seen at the present time, upon questions of cost, and it will vary very much with the character and the speed of the vessel. Generally speaking, it would seem that in very slow vessels, where the velocity of the rotation of the propeller must necessarily be very slow, the pressure range mentioned in the paper will be the best. If the line of demarcation was put higher with such low vessels as are dealt with primarily in this paper, the weight and the cost go up very materially, and the turbine becomes rather abnormal; the blades at the entry become very short, and the economy is not improved. On land, and for fast vessels where the velocities of rotation are very much greater, a compound engine and a turbine with a pressure at entry of about 30 lbs. would probably give the best all-round results. We are very much obliged to Professor Weighton for his critical remarks, which are to the point. The diagram of the reciprocating engine on page 79, is taken from an engine at Charlottenburg. Professor Weighton is quite right in remarking that the diagram should have referred to lbs. of steam per brake-horse-power rather than indicated-horse-power. As Mr. Mark Robinson pointed out, it had been established by the late Mr. Willans and others years ago that there was no advantage in a better vacuum than 25 or 26 ins. for reciprocating engines generally, the reason, of course, being that the low-pressure piston cannot be made large enough. If it is made sufficiently large to accommodate the expansion, then the losses by frictional resistance set up by the momentum of the very heavy weights that have to be set in motion with each revolution, especially of the very large low-pressure piston, absorb all the additional power that is given by increased expansion of the steam. With a turbine there is no limit to the expansion ratio which can be dealt with. There is practically no additional friction introduced by adding rows of blades. The surface velocity can be made very high, and very few rows serve the function of increasing the expansion ratio to any desirable extent to meet a very high vacuum in the condenser.

Mr. Morcom criticised the paper by saying that there were not enough accurate or scientific data as regards the efficiencies of the high-pressure and low-pressure portions, if I understood him correctly. It is a very difficult matter to set out such figures in a concise manner in the time and space available. In the first place, there is the water of condensation, which will amount to perhaps 12 per cent. in the steam entering the low-pressure turbine. This cannot be drained out efficiently, especially on board ship. You cannot have very large separators on board a ship, and this is not only a loss in volume of steam, but frictional losses are also caused thereby. As far as we know, the efficiency of the turbine in terms of the steam in a gaseous state that is passing through it may be between 60 and 75 per cent. Mr. Bell has put one view of the question clearly—that adding the turbine to the reciprocating engine has the effect of diminishing the temperature range in the cylinders of the reciprocating engine, and therefore of reducing the steam consumption by reducing the condensation. This, however, relates chiefly to the high-pressure cylinder.

With regard to the diagrams on page 79, I have received* from Professor Weighton a diagram (reproduced on page 94) representing the consumption of steam per brake-horse-power of the quadruple-expansion condensing engine at the Armstrong College, Newcastle-on-Tyne. This diagram gives the curve of consumption of a steam turbine in lbs. per K.W. hour for various vacua reproduced from Fig. 3, in direct comparison with the consumptions obtained by Professor Weighton. These latter consumptions have been reduced in this diagram to the same units as for the turbine—viz., lbs. per K.W. hour—by taking an overall efficiency of dynamo of 93 per cent.

* This paragraph and the next were added subsequently to the meetings.—ED.

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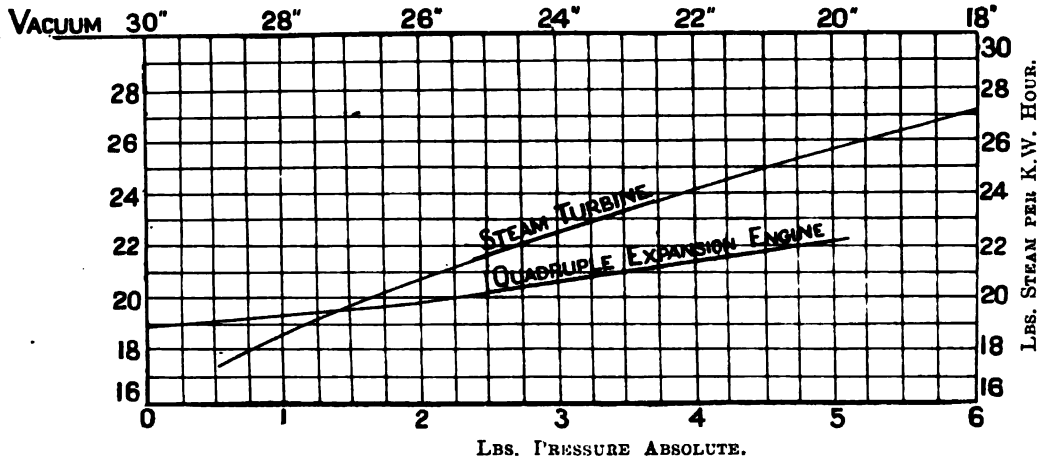
In regard to the communication sent by Major Ferretti, although wishing to favour any proposals tending generally to improve steam turbine propulsion, yet it seems desirable to point out clearly the inherent difficulties that have to be faced. The system of propulsion by jets fed by turbine-driven pumps is undoubtedly, at first sight, very attractive. If, however, it is analysed closely, some adverse facts present themselves. In the first place, pumps driven directly by steam turbines without gearing only exist for high lifts; for low lifts, pumps driven by reciprocating engines directly, or by high-speed engines and turbines through intermediate gearing, are exclusively used, and the limitations of turbine pumps existing on land apply equally to ships; for to propel a ship by jets with efficiency, very large volumes and low lifts and velocities are essential, and for such a purpose a high-lift pump is quite inadmissible. A high-velocity jet of small volume for propulsion would involve excessive slip and loss of propulsive power in proportion to the horse-power expended by the turbine in driving the pump. A large volume of water is therefore necessary. A quantity approaching that passing and operated on by the ordinary screw must be passed through the pump and jets, and this great volume involves a further loss over and above the skin friction of the water in the passages and pumps—that is, the great weight of water that has to be carried inside the ship, which considerably increases the dead weight. These are a few of the unfortunate drawbacks to jet propulsion, and it is a pity that the balance appears to weigh against a system possessing

DIAGRAM SHOWING EFFECT OF VACUUM ON STEAM CONSUMPTION.



so many good points. In regard to the view expressed by Colonel Nabor Soliani that in warships reciprocating engines might be provided for 30 to 40 per cent. of the total power, and turbines for the remainder, we may say that the question seems to us to involve many considerations, such as the efficiency at full speed, the efficiency at the lower and cruising speeds, the total weight and the handiness and reliability of the machinery generally; consequently we feel it would be impossible to deal adequately with this branch of the subject within the scope of our reply on this occasion.

Mr. R. J. WALKER (Member): My Lord and Gentlemen, I think Mr. Parsons has dealt with most of the questions raised in the discussion, but I should like to supplement his remarks on one or two points. I agree with Mr. Parsons that we fully appreciate Professor Weighton's criticism on the diagram referred to. The diagrams Nos. 2 and 3 were put in merely to illustrate the effect of vacuum on steam consumption as obtained in a reciprocating engine and with a turbine, and it will add to the value of the paper and the discussion, to have the particulars of the experiments Professor Weighton refers to. The initial pressure of the turbine has been dealt with in the paper. In marine work there does not appear to be any object in going to a much higher pressure than 15 lbs. absolute. The main consideration is one of relative cost. In regard to the question of the most suitable type of reciprocating engine

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to adopt, working in conjunction with a turbine, it may be said that the principal object of the combination system is entirely one of economy, and as to whether a compound or triple-expansion engine should be adopted, we must be guided more or less by the practice of reciprocating engine builders. The main point is to get the most economical reciprocating engine for a given back pressure. The division of power estimated as being developed by the reciprocating engine and the turbine respectively has been given in the table referred to in the paper. In columns B and C (page 82) it will be noted that the estimated powers developed by the reciprocating engine and the turbine respectively are 6,300 and 2,000. It would appear that with an initial pressure of 7 lbs. absolute the turbine could develop about 25 per cent. of the total power of the combination, and with 15 lbs. absolute about 33 or 34 per cent. of the total power.

Mr. Allen has referred to the question of the efficiency of the propellers. The most attractive proposal for the combination appears to be its application to intermediate liners. The original intention of Mr. Parsons was to fit the combination into purely cargo boats, and, as explained in the paper, it was at a somewhat later date that it was thought the combination would be of very great advantage in the intermediate type of liner of moderate speed. These latter vessels are of large dimensions, and the lines are fairly fine, and I do not therefore think there would be any great difficulty in obtaining reasonable propeller efficiency. In a purely cargo boat of full form aft, I agree with Mr. Allen that probably the best arrangement would be to fit one reciprocating engine in the centre line of the vessel, as is usually fitted in that class of ship, with two low-pressure turbines, one on each side of the ship. This would enable you to get the propellers of the turbines well out, and obtain a better flow of water on to the smaller propellers. Experiments have been made with these fast-running propellers in combination with the slower-running propellers of the reciprocating engines by Messrs. Swan & Hunter on their experimental launch which they used in connection with the express Cunarders. The after part of the model was modified to suit different dispositions of propellers, and from the result of these experiments it would appear that practically the same efficiency with the turbine-driven propellers might be expected as would be obtained with propellers of an all-turbine arrangement.

Mr. Bevis has referred to the question of oil coming over from the reciprocating engine to the turbine. This is a point which has been carefully considered. At the present time, in all war vessels the exhaust from the auxiliary machinery is carried into the turbine, and this does not appear to have any detrimental effect on the blading. However, provision is being made in these low-pressure turbines to increase the width of the initial blades so as to minimise any filtration effect should oil come over from the reciprocating engines. In these days of well-designed reciprocating engines, however, we do not anticipate that much internal lubrication of the engine should be required. From inquiries made regarding combination installations fitted on land, and after some years of running, there does not appear to be any settling of oil on the blades. As regards the question of additional weight required for extra strength for reversing raised by Mr. Bevis, this, I think, is already allowed for; but, in any case, there is a very fair margin in the estimated saving of weight at present. Turning to the table on page 82, it will be found that for 1,000 more horse-power in the combination there is a saving of 65 tons, and if we take the estimated additional weight required, should quadruple engines and boilers be fitted of the same total power as the combination, there is a still greater margin.

THE PRESIDENT: Gentlemen, I think we must pass a hearty vote of thanks to Mr. Parsons and Mr. Walker for their interesting paper, which has led to a most instructive discussion.

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SPEED TRIALS AND SERVICE PERFORMANCE OF THE CUNARD TURBINE STEAMER "LUSITANIA."

By THOMAS BELL, Esq., Member of Council.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 9, 1908 ; the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D., President, in the Chair.]

IT is with feelings of some diffidence that I place before the members of this Institution this brief record of the trials and running in service of the Cunard turbine steamer *Lusitania*, for, apart altogether from the fact that the propelling machinery consists of Parsons turbines, and is a further practical exemplification of the Hon. C. A. Parsons' wonderful invention, the leading proportions of the design of the ship and machinery are the outcome of most careful deliberations on the part of the technical staff of the Cunard Company, of the Turbine Committee, of the Hon. C. A. Parsons and his assistants, of the British Admiralty, of Lloyd's Committee, and of the Board of Trade, in conjunction with the staff of the three firms entrusted with the design and construction of the two express Cunarders, so many of whom are distinguished members of this Institution. Very full reports of the trials have been published in the technical press, but your Council considered that the subject had not yet been exhausted, and it is hoped that this brief paper may furnish material for an interesting discussion on some points connected with this great Cunard enterprise.

As already described so fully in the technical and other journals, and as shown on accompanying diagrammatic plan, Fig. 1 (Plate IX.), the turbine machinery of the *Lusitania* consists of two pairs of compound turbines, a pair consisting of one high-pressure and one low-pressure unit, each of which actuates a line of shafting, so that there are four lines in all. The high-pressure turbines drive the wing shafts, and the low-pressure turbines the centre shafts. At the forward end of the low-pressure turbines are placed the astern turbines. This particular arrangement, in which the two centre propellers are used for manœuvring purposes, was forced on the designers by exigencies of space as the only possible one, and has proved most satisfactory.

The boilers, as will be seen from Fig. 2, are divided into four equal groups in separate watertight compartments. The coal bunkers are situated only at the sides

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in the three after boiler rooms, but in the forward boiler room, owing to the fineness of the ship, the capacity of the side bunkers is comparatively small, and a large athwartship bunker becomes necessary, in addition to those at the sides.

The following are the principal dimensions and particulars of the turbines, condensers, shafting, and boilers :—

ENGINES.

Turbines.	Diameter of Rotor.	Length of Blades.	
		In 1st Expansion.	In Last Expansion.
H.P.	96 in.	$2\frac{3}{4}$ in.	$12\frac{3}{8}$ in.
L.P.	140 „	$8\frac{1}{4}$ „	22 „
Astern	104 „	$2\frac{1}{4}$ „	8 „

Total cooling surface, main condensers = 82,800 sq. ft.
Area of exhaust inlets ... .. = 158 sq. ft. each.
Bore of circulating discharge pipes = 32 in.
Diameter of tunnel shafts ... = 20 in. external, 10 in. hole.
„ of propeller shafts ... = 22 in. external, 10 in. hole.

BOILERS.

Working pressure, 195 lbs. per sq. in.
23 double-ended boilers, 17 ft. 6 in. mean diam. by 22 ft. long.
2 single-ended boilers, 17 ft. 6 in. mean diam. by 11 ft. 4 in. long.
Total number of furnaces ... ... = 192.
Total grate surface ... ... = 4,048 sq. ft.
Total heating surface ... ... = 158,352 sq. ft.
Total length of boiler rooms ... = 336 ft.
Total length of main and auxiliary engine rooms ... ... = 149 ft. 8 in.

With regard to the running of this large machinery and boiler installation, it may be of interest to members to give detailed particulars of the forced lubrication system, and of the system of feed heating and supply to the boilers. Each of these systems demanded that adequate provision be made to meet all possible contingencies, and thus enable the engineers in charge to be free to attend to the numberless duties connected with the supply of electric power and lighting, the supply of hot and cold, fresh and salt water throughout the ship, the pumping of the bilge and of the ballast compartments, and last, but not least, the supervision of the large army of stokers or firemen and trimmers, and the regulation and distribution of the coal supply from the various bunkers.

Regarding the forced lubrication system, of which a diagrammatic sketch is given

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in Fig. 3, the following statement gives the weights of the various revolving parts, together with the size of bearings and the pressure on same :—

Weight of one H.P. turbine rotor complete ... = 86 tons.
„ „ L.P. „ „ „ ... = 120 „
„ „ Astern „ „ „ ... = 62 „

—	Main Bearing Journals.		Pressure per square inch of Bearing Surface.	At 190 Revs. Surface Speed of Journal.
	Diameter.	Effective Length.		
H.P. Rotor	27⅛ in.	44¾ in.	80 lbs.	1,350 ft. per min.
L.P. „	33⅛ „	56½ „	72 „	1,650 „ „
Astern „	24⅛ „	34¾ „	83 „	1,200 „ „

A large main drain or reservoir tank is placed amidships low down in the engine room, in such position that the oil from all the bearings readily flows into it; the oil is pumped from this tank by four direct-acting single-cylinder pumps, through a system of filters, to gravitation tanks placed in the engine-room casings about 26 ft. above the bearings. These tanks are of sufficient capacity to maintain the oil supply to the bearings for ten or eleven minutes after the complete stoppage of all the pumps. There is a separate and distinct system of supply and gravitation pipes to the bearings on each side of the ship, with cross connections, so that, in the event of any defect arising on one side, it can be at once shut off, and the whole supply to all the turbines directed through the other side. Gauges showing the pressure in the oil pump discharges to the filters, and also the pressure in the gravitation pipes to the main bearing journals and thrusts, are fitted up at the starting platform as well as at the pumps, these latter being placed in an easily accessible part of the centre engine room. A pressure gauge is also fitted at each bearing, and, in addition, the drain or discharge from the bottom reservoir from each bearing and thrust is led through a glass-sided lantern-shaped receiver, so as to make the flow visible, and a thermometer fitted up at this point enables the temperature of the oil from each of the twelve main bearings and four thrust blocks to be taken and recorded, the practice being to log the temperature hourly. In the gravitation tanks are placed copper coils of a total surface of 1,335 square feet, through which cold sea water is circulated, and this surface suffices to maintain the 4,700 gallons of oil which is in circulation, at a temperature not exceeding 30° above that of the engine room. Each coil is self-contained and withdrawable, so that a leakage of sea water from pitting or other cause can readily be located and repaired without interfering with the general working of the oil supply. There are also reserve oil tanks having a total capacity of 4,200 gallons for use in case of emergency.

Three of the six oil pumps, the discharges from which are all cross-connected, are

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ample for all the necessary oil supply, but four are kept in use, so that no risk is run in the event of the failure of any single pump not being at once noticed.

With reference to the feed system, shown diagrammatically in Fig. 4 (Plate IX.), it should be explained that all the auxiliaries in the ship, excepting the turbo-generators and the evaporators, exhaust into a general system of auxiliary exhaust piping connected to the surface feed heaters, auxiliary condensers, and atmosphere. The turbo-generators exhaust into an entirely distinct system of piping connected to the contact feed heaters, auxiliary condensers, and atmosphere. From Fig. 4 it will be seen that the hotwell pumps draw from the hotwell and discharge through two feed filters to the two surface feed heaters, and thence to the contact heaters; these latter constitute the usual feed receivers, and are fitted with float gear, controlling not only the steam supply to the main feed pumps, but also to the hotwell pumps. In addition, the hotwell pumps are controlled by float gear in the hotwell tanks—the latter, in case of a shortage of water supply, to prevent the pumps running away, and the former precluding the possibility of water being forced back to the turbo-generators, should the non-return valve in the steam inlet to the contact heater fail to act in the event of the feed pumps suddenly slowing.

In most large merchant ships, on account of accidents which have occurred, it has been the general practice to lead the exhaust from the electric engines direct to the main or auxiliary condensers only, to ensure steady running of the dynamos and to prevent the possible passing back of water from other auxiliary engines into the cylinders of the electric engines. In this present case the electric installation is so large, and the temperature of the hotwell, due to the high vacuum maintained, so low, that it was considered to be essential to utilise this exhaust for the heating of the feed water, and, by adopting this duplicate system of feed heaters and hotwell control, this end has been attained with a minimum of risk.

Owing to the very great length of feed supply pipes from the feed pumps to the forward boilers, there was considerable speculation as to whether the feed supply to each boiler room would have to be entirely distinct. Had this been so, each pair of feed pumps would have required to be worked entirely independently of the others, and, in the event of any hitch occurring, one of the two pairs of stand-by pumps would have had to be at once connected to the particular boiler room in question. In actual running, however, it was found that the four main feed supply pipes could be cross-connected and made common, thus making any demand for special care and vigilance on this score unnecessary.

With regard to the maintenance of steady steam at sea, although innumerable types of rocking and self-cleaning firebars have been devised and tried in connection with automatic stokers, none of these has been found to give satisfactory results in merchant vessels. The stoking, and especially the cleaning of the fires wherever hard steaming is required, is the same as it was fifty years ago, and, as a consequence,

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is dependent on the willingness and ability for work of each unit of the stoking complement of the ship.

The following hourly abstract of one of the watches on the *Lusitania* brings home to one's mind the loss in steam and speed caused by cleaning fires, especially when the coal is small. It can easily be calculated from this what an appreciable increase in the ship's mean speed could be obtained from this cause alone, if the price and supply would admit of the use of some system of oil fuel burning.

—	Mean Revolutions.	Corresponding Speed of Ship.	
First hour	178	about 24 knots	24·15
Second „	181	„ 24·3 „	
Third „	186	„ 25·0 „	25·05
Fourth „	187	„ 25·1 „	
Mean for watch ...	183	„ 24·6 „	

Regarding the observations from readings taken in the engine-room on the official trials generally, it may be stated that on the measured miles the revolutions were obtained from electric records in connection with the pallograph apparatus, but on the lengthened trials they were taken from half-hourly readings of the engine-room counters. The vacuum recorded is that of the vapour in the main exhaust orifice forming the top of the condensers, and as measured by a syphon mercury gauge, the readings of which throughout are corrected to correspond to a 30 in. barometer. The total quantity of feed water is obtained from hourly counter readings of the double strokes of the Weir's feed pumps, the average length of stroke and the slip or leakage of each pump being determined, both before and after the trials, by careful tests.

The consumption of steam of the auxiliary machinery is obtained by noting the amount by which the temperature of the total feed water was raised in the feed heaters, and to the amount thus found must be added the steam used in the turbo-generators, the exhaust from which was led direct to the auxiliary condensers on the official trials. As before stated, in actual service these turbo-generators exhaust into the contact heaters, and thus raise the feed temperature to about 200°. These connections had to be slightly altered at the time of the trials, and unfortunately, therefore, advantage could not then be taken of this additional source of economy.

The torque horse-power was obtained by the Denny-Johnson apparatus, and the records show that, while a propulsive efficiency of the whole installation was obtained which accorded with the original estimate, the steam consumption of the turbines themselves was very satisfactory. It need hardly be pointed out that those two, viz., propulsive efficiency and steam consumption per unit of power, form an excellent

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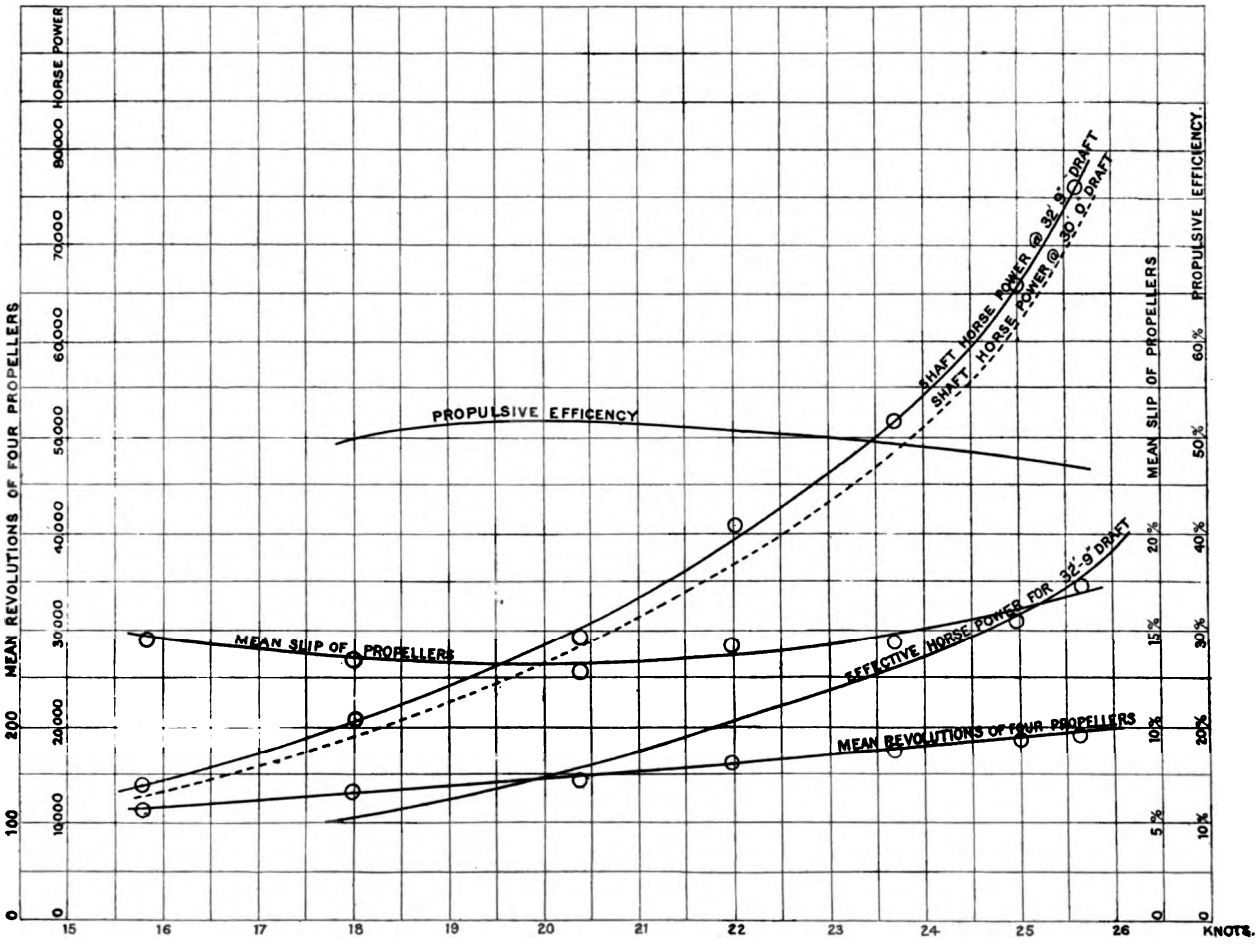
check on each other, for, whatever would unduly favour one, would be at the expense of the other.

The *Lusitania* was floated out of dry dock at Liverpool on July 22, 1907, and was thereafter coaled by the Cunard Company, the bunkers for the forward and after boiler rooms being filled with South Wales coal, and those of the two middle boiler rooms with Yorkshire coal.

PROGRESSIVE TRIAL OFF SKELMORLIE, JULY 27, 1907.



On her return to the Clyde, on the morning of July 27, a series of progressive runs was made on the Skelmorlie measured mile, as recorded on Table I. (at end of paper), with the ship at a mean draught of 32 ft. 9 in. These results are also given in graphical form in the diagram shown above, which gives curves of shaft horse-power, revolutions, and slip, on a common speed base. Two other most interesting curves have been added, one showing the effective horse-power determined by means of tank experiments, and the other showing the propulsive efficiency. It was intended to repeat this series of progressive runs at the termination of the official trials at a

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mean draught of about 30 ft. Unfortunately, however, thick weather on the morning of August 2 prevented this being carried out, but the dotted curve on the previous page indicates with sufficient accuracy what might have been expected.

On the evening of the 27th the *Lusitania* proceeded on a pleasure cruise round Ireland, during which consumption trials at 18, 21, and 23 knots were carried out, and, after landing the guests in the forenoon of the 29th, the vessel returned to the Clyde, making a consumption trial at 15¾ knots *en route*, the results of these trials being given in the first four columns of Table V. After checking the draught of ship, &c., the 48 hours' full speed continuous trial was commenced at midnight. This trial consisted of two double runs on a course of 304 knots between Corsewall Point and the Longships, and the results obtained are recorded on Table II. and the last column of Table V. The mean draught at starting was 32 ft. 7 in., and at the finish about 30 ft. 8 in. The coal consumed in the 50 hours during which the engines were running at full speed was found by measurement of bunkers to be about 2,200 tons. This represents an evaporation of 10·1 lbs. of water per lb. of coal from 165° temperature of feed, or 11·1 lbs. from and at 212°, and a consumption of coal for all purposes of 1·43 lbs. per shaft horse-power per hour, with a rate of combustion of coal of 24·3 lbs. per square foot of grate surface per hour. The number of stokers on watch was the same as in actual Atlantic service, and the air pressure in the ash-pits did not exceed ¾ in. of water column. The port evaporators were used for 10 hours of the trial, but, as the vapour from these was condensed in the port auxiliary condenser to which the exhaust from one set of the turbo-generators was led, they were discontinued and the make-up feed obtained from the reserve tanks for the remainder of the time.

The third trial, recorded on Table III., which was commenced in the forenoon of August 1, consisted of one double run between Corsewall Point and the Chicken Rock, a distance of 59 knots each way, but comparison with the dotted curve on page 101 shows that the tide conditions during this trial give altogether too favourable a speed result. The vessel was then headed for Ailsa Craig, and carried out the specified six runs between Ailsa Craig and the Holy Isle (off Arran). These latter, recorded on Table IV., which were run at a mean draught of about 30 ft. 2 in., give a very reliable record of power and speed at this draught, when compared with the 25·62 knots obtained on the measured mile at 32 ft. 9 in. draught. On the following day weather conditions precluded any further trials, and the reversing trial and the steering and circle turning trials were accordingly carried out on the vessel's passage to Liverpool on August 26.

In a fast passenger liner such as the *Lusitania*, it is of the utmost importance that the manœuvring capabilities should leave nothing to be desired, and to demonstrate the possibilities of the ship in this respect, various trials were made, the most important being the following :—

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STOPPING TRIAL.—The ship was run on the Skelmorlie measured mile at a speed of 22·8 knots, the average revolutions of the propellers being 166 per minute. On entering the mile, the engine-room telegraphs were rung to "Full speed astern"; the ship was brought to rest in 3 minutes 55 seconds, the distance run being about three-quarters of a mile, or about six times the length of the ship. During this trial the boilers in the three after boiler rooms only were in use, and the initial pressure at the astern turbines was about 90 lbs.

CIRCLE TRIALS.—With the ship initially on a straight course and the turbines running at an average speed of 180 revolutions per minute, the steering wheel was put hard over in 17 seconds. The tiller went over to 35° in 20 seconds and the vessel made a complete circle in 5 minutes 50 seconds, the average revolutions coming down at the completion of the circle to 70 per cent. of the rate at the commencement. The resulting circular path was approximately 1,000 yards, or four lengths of the ship, in diameter. This manœuvre was made both under starboard and port helm with very closely confirmatory results.

Going astern with the inner propellers running at a uniform rate of 136 revolutions per minute and under full helm, resulted in half circles being made in an average time of 6 minutes 45 seconds.

As important factors contributing to these very satisfactory results, it may be remarked that, following on the suggestion of Sir Philip Watts, the deadwood aft is cut away in a fashion similar to that in recent warships. The inner propellers are fairly close together, and as the rudder is of large dimensions in the fore and aft direction, the race from these propellers impinges fully upon it when any helm is used. The vessel, consequently, is very similarly circumstanced to a single screw ship, or a triple screw ship with all three propellers in action, and gets steerage way without any perceptible headway, and this feature was very noticeable during the steering trials. At first sight it would appear that the outer propellers should have been those utilised for manœuvring purposes, as the outer shafts have about three times the spread from the middle line that the inner shafts possess. For turning with propellers alone without the help of the rudder they would have been much the more effective, but they would not have possessed any such advantage as that alluded to above in respect of obtaining steerage way without headway.

Table VI. has been compiled for comparison with Table V. to show the additional consumption of steam for auxiliary purposes under actual working conditions at sea with the ship full of passengers. This shows very clearly the demand which modern improvements make on the steam, and hence coal consumption, of a large passenger vessel. An additional line has been added to Tables V. and VI. to show total coal consumption on a voyage of 3,100 nautical miles at the various speeds and under the different conditions.

With reference to the third voyage West, from November 2 to November 8 of last year, thanks to the courteous permission of the Chairman of the Cunard Company,

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the leading particulars of the official engine-room log are summarised in Table VII. Regarding the mean draught of the vessel at sea, it may be remarked that, after the second day out, certain of the forward tanks were gradually filled for the purpose of avoiding excessive trim, so that the mean draught on November 5, 6, and 7 was approximately 32 ft., or very little more than the mean of the first pair of runs from Corsewall Point to the Longships and back. The conditions, however, were otherwise very different, for, with the exception of the twelve hours of fine weather and smooth sea from noon till shortly after midnight on November 6, it was throughout the average Mid-Atlantic winter weather—namely, strong winds and resulting boisterous sea. Up till midnight on the 6th, *i.e.*, for 2,176 knots out of a total of 2,781 knots, the mean speed works out at 24·65 knots per hour; but, unfortunately, early on the 7th the wind freshened, gradually increasing to a furious south-west gale, which reached its height about 4 p.m., and reduced the average speed for the last 24 hours below 23 knots per hour, and thus brought down the mean average for the completed voyage to 24·25 knots per hour. Table VIII., giving the mean average speeds at the different stages of the voyage, shows very clearly the effect of this gale, unfortunate so far as preventing the vessel from complying with the contract conditions, but giving those connected with the ship an opportunity of thoroughly satisfying themselves as to her behaviour when driving through the huge waves at about 22½ knots, without any racing of engines or sign of labouring, and dispelling the idea, current in some minds, that turbine propelled ships do not show to advantage in heavy weather.

The following particulars of the steam consumption are given in conjunction with the figures of coal consumption set forth in Table VII. Throughout the voyage a careful record of the feed pump counters gave an average of 998,000 lbs. of water pumped into the boilers per hour. Of this, about 114,000 lbs. was used by auxiliary machinery exhausting into the feed heaters, 26,000 lbs. by the evaporating plant supplying feed make-up and washing water, and about 6,500 lbs. for steam to the thermo tanks, galleys, and pantries, both of which latter figures are based on data obtained from tests carried out before the vessel left the Clyde. Hence, taking the average shaft horse-power as 65,000, the steam consumption per shaft horse-power hour works out as follows:—

			Per shaft horse-power hour.
Main turbines	851,500 lbs.	=	13·1 lbs.
Auxiliary machinery	114,000 „	=	1·75 „
Evaporating plant and heating	32,500 „	=	·5 „
	998,000 lbs.		15·35 lbs.

Average amount of coal burnt per hour for all purposes = 43½ tons.
Water evaporated per lb. of coal = 10·2 from a feed temperature of 196°.
„ „ „ „ = 10·9 from and at 212°.
Coal for all purposes per shaft horse-power per hour = 1·5 lbs.
Coal per square foot of grate per hour = 24·1 lbs.

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Taking a mean displacement of 36,000 tons, this represents at 24½ knots per hour a consumption of almost exactly 11 lbs. of coal per 100 nautical miles, per ton of displacement. The coal used was half South Wales and half Yorkshire, practically the same as on the official trials.

In the light of present-day experiences, the adoption of turbines, even on such an immense scale, appears very different to what it did in the summer of 1903, and too much praise cannot be awarded to the late Lord Inverclyde for his courage and far-sightedness, in this as well as in so many other matters, and also to the Cunard Board and their technical adviser, Mr. James Bain, for so heartily supporting and furthering his progressive views. The largest turbines then constructed by the Turbinia Works were those in the now historic *Queen*, built by Messrs. Denny, the pioneers of turbine-driven merchant vessels, in which the largest unit weighed some 35 tons, and to suggest jumping at one bound from a 35-ton unit to one of between 400 and 500 tons was most daring. The step has, however, been safely made, and the Hon. C. A. Parsons' genius, backed by sound British workmanship, has fully justified the demands made on it and the confidence placed in the possibilities of his marvellous invention.

R.M.S. "LUSITANIA."

TABLE I.

PROGRESSIVE TRIAL OFF SKELMORLIE, JULY 27, 1907.

Draught 32 *ft.* 9 *in.*, *Displacement* 37,080 *tons.*

Time.	Pressures.		Vacuum at 30 in. Barometer.	Revs. per Minute.	Speed in Knots.	Shaft Horse-power.	Slip of Propellers.
	H.P. Recr. lbs.	L.P. Recr.					
1st Double Run ..	157	5¼ lbs.	28″	194·3	25·62	76,000	17·2 %
2nd ,, ,, ..	135	2½ lbs.	27·9″	186·0	25·0	65,500	15·5 %
3rd ,, ,, ..	110	½ lb.	28·1″	174·2	23·7	51,300	14·5 %
4th ,, ,, ..	90	3½ Vacm.	28·1″	161·5	22·02	40,500	14·3 %
5th ,, ,, ..	70	6½″ Vacm.	28″	147·6	20·4	29,500	13·1 %
6th ,, ,, ..	50	10½″ Vacm.	28″	131·1	18·0	20,500	13·7 %
7th ,, ,, ..	35	14½″ Vacm.	28·1″	116·1	15·77	13,400	14·6 %

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TABLE II.

FORTY-EIGHT HOURS' FULL-POWER TRIAL BETWEEN CORSEWALL AND LONGSHIP LIGHTS, JULY 30 TO 31, 1907.

Mean Draught 31 *ft.* 7½ *in., Mean Displacement* 35,600 *tons.*

Time.	Pressures.		Vacuum at 30 in. Barometer.	Revs. per Minute.	Speed in Knots.	Shaft Horse-power.	Slip of Propellers.
	H.P. Recr. lbs.	L.P. Recr. lbs.					
1st Run	146	2½	27·9″	188·8	26·35	70,400	—
2nd „	145	2½	28″	187·4	24·3	68,200	—
3rd „	146	2½	27·9″	187·5	26·3	68,700	—
4th „	148	2½	27·8″	187·9	24·6	68,100	—
Mean of means ..	146 lbs.	2½ lbs.	27·9″	187·9	25·4	68,850	15 %

TABLE III.

FULL-POWER TRIAL BETWEEN CORSEWALL AND CHICKEN LIGHTS, AUGUST 1, 1907.

Draught 30 *ft.* 4½ *in., Displacement* 34,160 *tons.*

Time.	Pressures.		Vacuum at 30 in. Barometer.	Revs. per Minute.	Speed in Knots.	Shaft Horse-power.	Slip of Propellers.
	H.P. Recr. lbs.	L.P. Recr. lbs.					
1st Run	152	2¾	28″	191·3	26·75	72,000	—
2nd „	152	2¾	28″	191·7	26·17	72,800	—
Mean	152 lbs.	2¾ lbs.	28″	191·5	26·46	72,400	13·2 %

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TABLE IV.

FULL-POWER TRIAL BETWEEN AILSA CRAIG AND HOLY ISLE, AUGUST 1, 1907.
Draught 30 *ft.* 1 *in.*, *Displacement* 33,770 *tons.*

Time.	Pressures.		Vacuum at 30 in. Barometer.	Revs. per Minute.	Speed in Knots.	Shaft Horse-power.	Slip of Propellers.
	H. P. Recr. lbs.	L. P. Recr. lbs.					
1st Run	151	2¾	28″	191·3	25·62	72,500	—
2nd „	152	2¾	28″	191·1	26·36	72,000	—
3rd „	153	3	28″	191·9	25·31	72,000	—
4th „	147	2½	28″	191·0	26·16	72,100	—
5th „	149	2½	28″	190·2	25·26	70,800	—
6th „	149	2½	28″	191·2	25·95	71,800	—
Mean of means ..	150 lbs.	2¾ lbs.	28″	191·2	25·77	71,910	15·3 %

TABLE V.

Actual steam and coal consumption of main and auxiliary engines at various speeds under conditions prevailing on official trials, viz., turbo-generators exhausting to auxiliary condensers, other auxiliaries exhausting to heaters.

Shaft horse-power	13,400	20,500	33,000	48,000	68,850
Speed in knots..	15·77	18·0	21·0	23·0	25·4
Total consumption of Auxiliaries in lbs. per hour..	71,000	76,400	85,700	96,700	116,500
Total consumption of Turbines in lbs. per hour	284,500	353,600	493,300	668,300	879,500
Steam consumption of Auxiliaries in lbs. per H.P. hour	5·3	3·72	2·6	2·01	1·69
Steam consumption of Turbines in lbs. per H.P. hour	21·23	17·24	14·91	13·92	12·77
Total steam consumption in lbs. per H.P. hour	26·53	20·96	17·51	15·93	14.46
Temperature of feed water	200°	200°	199°	179°	165°
Coal consumption in lbs. per H.P. hour ..	2·52	2·01	1·68	1·56	1·43
Estimated coal consumption in tons on a voyage of 3,100 nautical miles, allowing 20 tons for galleys, &c.	2,980	3,190	3,670	4,520	5,390

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TABLE VI.

Estimated steam and coal consumption at various speeds allowing for the additional auxiliary steam consumption found requisite under actual service conditions for the washing water supply, &c., with a full complement of passengers, weather conditions being as on official trial.

Shaft horse-power	13,400	20,500	33,000	48,000	68,850
Speed in knots	15·77	18·0	21·0	23·0	25·4
Total consumption of Auxiliaries in lbs. per hour	93,500	100,900	112,700	127,500	149,700
Total consumption of Turbines in lbs. per hour	284,500	353,600	493,300	668,300	879,500
Steam consumption of Auxiliaries in lbs. per H.P. hour	6·97	4·92	3·41	2·65	2·17
Steam consumption of Turbines in lbs. per H.P. hour	21·23	17·24	14·91	13·92	12·77
Total steam consumption in lbs. per H.P. hour	28·2	22·16	18·32	16·57	14·94
Temperature of feed water	200°	200°	200°	200°	200°
Coal consumption in lbs. per H.P. hour ..	2·76	2·17	1·8	1·62	1·46
Estimated coal consumption in tons on a voyage of 3,100 nautical miles, allowing 20 tons for galleys, &c.	3,270	3,440	3,930	4,700	5,490

TABLE VIII.

Date—1907.	Length of Steaming Day.	Distance run.	Speed.	Total Knots Steamed.	Total Time.	Mean Average Speed.
	Hrs. Mns.				Hrs. Mns.	
Noon November 3 ..	0 52	21 knots.	24·24	21	0 52	24·24
,, November 4 ..	24 57	606 ,,	24·28	627	25 49	24·27
,, November 5 ..	25 2	616 ,,	24·6	1,243	50 51	24·44
,, November 6 ..	24 55	618 ,,	24·8	1,861	75 46	24·57
Noon till midnight, November 6 ..	12 30	315 ,,	25·2	2,176	88 16	24·65
Noon, November 7 ..	12 22	295 ,,	23·85	2,471	100 38	24·55
Morning, November 8	14 2	310 ,,	22·09	2,781	114 40	24·25
	114 40	2,781 knots				

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TABLE VII.

ABSTRACT OF ENGINE-ROOM LOG FOR 3RD VOYAGE WEST: QUEENSTOWN TO NEW YORK.

Date when last Dry Docked, July 22, 1907. Mean Draught, leaving Queenstown, 33 ft. 7 in. Mean Draught, arriving New York, 30 ft. 10 in.

Date—1907.	Steam Pressures.			Temperatures.		Vacuum.	Barometer.	Length of Day.		Distance by Observation.	Mean Speed.	Mean Revs.	Mean Slip per cent.	Coal consumed for Main and Auxiliary Engines.
	Boilers. lbs.	H.P. Recrs. lbs.	L.P. Recrs. lbs.	Hot well.	Feed Water.			Hrs.	Mns.					
Noon, Nov. 3..	170	140·0	2·3	68°	200°	28″	30·4″	—	52	21 knots	24·24	182·5	16·5	40 tons.
,, ,, 4..	169·1	142·2	2·2	78°	197°	28″	29·7″	24	57	606 ,,	24·28	182·6	16·4	1,090 ,,
,, ,, 5..	167·3	140·6	2·3	78°	198°	28·2″	30″	25	2	616 ,,	24·6	182·8	15·4	1,090 ,,
,, ,, 6..	168·3	140·4	2·5	70°	196°	28·2″	30·1	24	55	618 ,,	24·8	183·5	15·1	1,090 ,,
,, ,, 7..	168·3	138·3	2·2	72°	195°	28″	29·6″	24	52	610 ,,	24·52	181·4	15·0	1,090 ,,
1.14 a.m. Nov. 8	165	132·5	1·5	75°	200°	27·8″	29·3″	14	2	310 ,,	22·09	174	20·2	576 ,, (This includes all coal used till 10 a.m. on the 8th.)
								Total		Total of				
Means	168	139·3	2·2	74·5°	197°	28·1″	29·8″	114	40	2,781 knots	24·25	181·1	15·9	4,976 tons.

SUMMARY OF TOTAL COAL CONSUMED ON VOYAGE:—Liverpool to Queenstown, 408 tons; Queenstown to New York, 4,976 tons; galleys, &c., 18 tons: Total coal taken from bunker from leaving Landing Stage, Liverpool, till moored at wharf, New York, 5,402 tons.

PASSAGE—QUEENSTOWN TO SANDY HOOK—4 DAYS, 18 HOURS, 40 MINUTES.

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DISCUSSION

The Hon. C. A. Parsons, C.B., M.A., F.R.S., D.Sc. (Member of Council): My Lord and Gentlemen, I do not think that it is realised what a great departure from precedent the building of these vessels involved, and with how much modesty Mr. Bell has described his share in the undertaking; he might have said a great deal more than he has done, because his part of the work was one of the most important. The Cunard Committee was appointed by the late Lord Inverclyde just after the first turbine Channel boat, the *Queen*, built by Messrs. Denny, and fitted with the largest set of turbines then in existence, had been put on service. The Committee, of which Mr. Bell was a member, and which I had the privilege of assisting, practically decided, on the strength of the performance of the *Queen*, and subsequently of the *Brighton*, the Newhaven and Dieppe boat which was run against her sister reciprocating boat, to go in for turbines. I do not think it is realised how great an amount of work actually had to be done by the contractors, and the very great departure in scale; because, if we look back into the history of engineering, I think we shall find that in most cases where great mistakes have been made, it has been where the scale of magnitude of the work has been largely increased, and the rules which have been found successful on the smaller-sized undertakings were adopted without sufficient thought or verification on the larger scale. This was, however, not the case in these large vessels. Every point was calculated and tested, and Mr. Bell did not take anything for granted. He constructed a set of marine turbine engines at Clydebank, coupled with electrical generators as dynamometers, and from these he worked out the efficiencies. He also investigated the difficult question of the glands of the turbines, and he arrived independently at the same conclusion as we had—that, at the surface speed which would necessarily be involved in these very large turbines, the method of packing with Ramsbottom rings adopted in the *Queen* would not work, and that the method we had universally adopted for land turbines for many years, namely, the labyrinth system, was indispensable. Mr. Bell constructed a working gland of the full size of the *Lusitania's* low-pressure gland, and thoroughly tested it before proceeding with the actual construction of the turbine.

When we look back on the work, it seems fairly easy, but there were undoubtedly very great risks involved in such a large departure, and a very important point of the work was to minimise and, if possible, avoid such risks. I remember suggesting that eight shafts might be adopted, so that the turbines would not have been very much larger than those in the *Queen* and *Amethyst*; but, from a naval architect's point of view, the eight shafts and eight propellers did not appear to be a satisfactory solution of the question, and it was thought better to adopt the larger turbines and the four shafts. If I may say so, I think, from the results of many turbine-propelled vessels, that the slip of the propellers of the *Lusitania* is slightly too low for maximum efficiency, for it has generally been found that 20 to 24 per cent. apparent slip ratio at the speed in question gives about the best results. It is probable that some gain in speed might result by slightly diminishing the size of the propellers concurrently with slightly increasing the pitch. However, it is far safer to make screws too large in the first instance, for, if made too small, cavitation arises, and the efficiency drops very rapidly in approaching full power. With the propeller on the large side to commence with, the efficiency rises slowly as you diminish the size, until a point is reached which gives maximum efficiency. If, on the other hand, the size is diminished too much, then the efficiency drops very suddenly, and the results are very disappointing. I think one cannot exaggerate the very great work that Mr. Bell and his colleagues, and also Mr. Laing and Messrs. Swan & Hunter have successfully achieved, under the direction of the Cunard Company, Mr. Bain, and the Cunard Committee, in the construction and design of these large vessels.

Mr. Andrew Laing (Member of Council): My Lord and Gentlemen, it has been most interesting to listen to Mr. Bell's paper and the very valuable information that he has put forward. It is not my intention to offer much criticism, but, as I was associated with the construction of the machinery of the

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Mauretania, it is not altogether inappropriate that I should offer my friend Mr. Bell congratulations on the suggestive paper he has read to this Institution. I also wish to second his well-expressed words of commendation to the Chairman and staff of the Cunard Company for the part they have taken in connection with these great ships. Mr. Bell's paper shows the shipowning and shipbuilding world that turbines on the Parsons principle can, beyond any possibility of doubt, be designed and constructed to give a very reasonable coal consumption. The machinery of the *Mauretania* does not differ in any very essential points from that of the *Lusitania*, but it may be as well to give some results obtained on trial by the *Mauretania*, not by way of comparison, but rather as a confirmation of those obtained with the *Lusitania*. Everyone will agree that the forty-eight hours' trial of each of the two ships represented Atlantic service conditions as near as was possible, without going beyond known distances, such as the Corsewall and the Longships, and on such runs the *Lusitania* made 25·4 knots, whilst the *Mauretania* made 26·04 knots. These results may be taken as reliable, seeing that the conditions as to draught, weather, and coal were similar and the same pilot was on board. I do not propose to compare the actual sea-going results, but I would say that when the two ships, staffs, and crew have settled down to their work in the no distant future, we may expect to see the vessels giving a good account of themselves in speed. Mr. Bell has done a great service to British engineers in placing before them so fully the trial results and sea performances of the *Lusitania*, and these results must be a great relief to many minds, especially on the question of turbines and their coal consumption for marine purposes.

Mr. W. J. Luke (Member of Council): My Lord and Gentlemen, there is really very little for me to say in the way of addition to the particulars that Mr. Bell has laid before you in this paper. On page 101 there is a reference to the progressive trials at Skelmorlie. The model was experimented upon for effective horse-power in the Haslar tank, and also in our own tank at Clydebank. At that time our tank was comparatively new, and the experiments we carried out were made more with a view of standardising our own apparatus than by way of a check on the Admiralty results, so that we relied almost entirely on the Admiralty results, and congratulated ourselves on the fact that our results agreed so closely with theirs. Taking all in all, I think that a propulsive efficiency of very nearly 50 per cent. at top speed, which is shown by the diagram, is a thoroughly satisfactory result to have obtained. It has been stated in some scientific papers, and no doubt quite accurately, that there are some reciprocating-engined ships which have obtained a propulsive efficiency exceeding 60 per cent.; but experiment undoubtedly shows that, with propellers of the type and proportions that are necessarily used in connection with the turbine, the propulsive efficiency is almost certain to be substantially lower than can be obtained in association with the reciprocating engine, and under those circumstances it appears to me to be vain to hope for very much better results than the efficiencies shown on the diagram. Passing on to page 104, there is a reference to the effect of the wind and sea on ocean speeds. The periodical *Schiffbau*, in the issue for June 28, 1905, gives some very interesting information on this point. In this article it is stated that with big fast ships, such as the *Deutschland* and *Kronprinz Wilhelm*, in a wind of force 7 and with corresponding sea, if the wind should be in a favourable direction, it prejudices the speed to the extent of 2 per cent., and with an unfavourable wind of this force it would lead to a reduction of speed of as much as 15 per cent. Under those circumstances, if a speed of 24½ knots can be kept up for two or three days in succession, I think it is a thoroughly satisfactory performance. I agree with what Mr. Laing has just said, that when both the ships—the *Lusitania* and the *Mauretania*—get into full working order there will be very little to choose between them, and I am sure we who are more closely associated with the *Lusitania* wish, if not quite as much success to the *Mauretania* as to the *Lusitania*, as least very nearly as much.

Vice-Admiral W. H. Henderson (Associate): My Lord and Gentlemen, I only want to ask the author of the paper one question. I have been to sea lately, and I believe that one or two practical engineers think that the coal consumption of these two vessels is very much greater than was estimated; that is to say, I was told, in discussing the question of the turbine *versus* reciprocating engines, that the consumption

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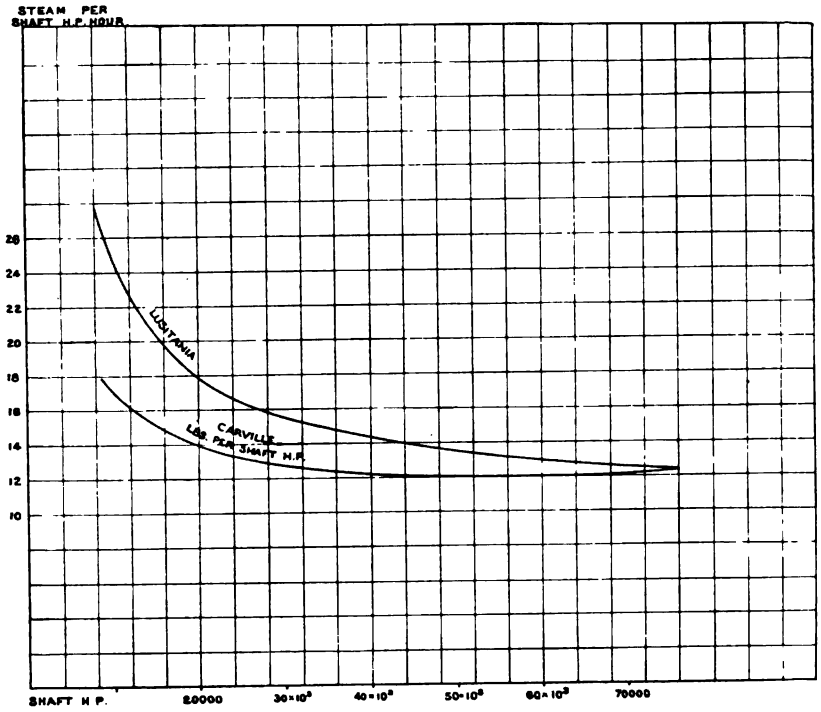
on these vessels was 1,350 tons instead of about 1,100 tons a day, as was originally expected, and that they really did not carry enough coal at this rate of consumption to carry them over at their proper speed to New York. I do not know whether this is true or not, but I should like an answer, because I think it is a question which affects the efficiency of the vessels and marine opinion generally.

Professor J. H. Biles, LL.D. (Vice-President): My Lord and Gentlemen, I am only able to say, with reference to this paper, how much this Institution is indebted to Mr. Bell and the Cunard Company and to Messrs. John Brown & Co. for giving us such full and valuable data. Seldom have data so full, so complete, and so valuable been given in connection with such a remarkable ship as has been given in this paper, and I hope the example set by these gentlemen will be followed by others. I also want to repeat what was said last year on Mr. Luke's paper—that it must not be forgotten that there is a connection between this Institution and these ships, the *Lusitania* and the *Mauretania*. Lord Brassey, one of our former presidents, urged very much upon the Government the question of subsidising mail steamers. As a result of that action, the Admiralty created a Departmental Committee, and asked this Institution to select a member to serve upon that Committee to advise the Admiralty in connection with this subject, and as a direct result of that Committee, I think, a subsidy was agreed upon which enabled the Cunard Company to build these two magnificent ships. The report of that Committee was based upon the terms of reference inquiring into the relation between the speed of mercantile ships and the subsidy that should be paid them. I happened to be the representative of this Institution on that Committee, and I feel very pleased at the result of our work in assisting in the creation of these vessels, and I can only hope that the amount of subsidy that the Government gave to the Cunard Company will amply repay them for their enterprise in building these vessels. The turbine part of it is the most interesting problem in engineering—certainly in modern engineering—that has ever been undertaken. It involves the relation between the propeller that wants to be big and to go slowly, and the turbine that wants to be small and to run fast, and in this case the combination has been made by compromise. How long we shall continue to be content with a compromise of that kind, time alone will show; but the mechanical difficulties that stood in the way of carrying out such a project were certainly enough to frighten most people, and I am not quite sure that those who undertook them were not rather alarmed at their own temerity; but they took their courage in both hands, and they deserve the reward of their success.

Diagram showing Comparative Steam Consumption of Electric Turbo-generators and of Marine Steam Turbines.



Mr. H. A. Mavor sent the following written communication:—Being specially interested in the application of electric propulsion to large steamships, the writer ventures to offer the following remarks

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from that point of view. Mr. Bell has presented to the engineering world a most instructive and valuable record of a unique power plant. The excellent results of this great experiment make it a mine of information for future workers, and the unreserved publication of the figures is an example of devotion to science of the highest order. A conspicuous feature of the tabulated results is the treatment of the auxiliary machinery and heating arrangements which, as shown in Table V., are charged with nearly 12 per cent. of the total steam at full power, and 20 per cent. of the total steam at one-fifth full power. This large proportion is consistent with a very high order of economy, as may be shown by a heat balance sheet for the whole plant.

"LUSITANIA" HOURLY BALANCE-SHEET IN MILLIONS OF B.TH.U. AT FULL POWER.

(*See* TABLE V., PAGE 107.)

Dr.			*Cr.*	
To Heat equivalent of work done by main engines, 68,850 H.P. × 2,545 B.Th.U. per H.P. hour =		174	By Coal at 1·43 lbs. per H.P. × 68,850 H.P. × 14,000 B.Th.U. per lb. ... ... =	1,380
Work done by auxiliaries by difference between total supply of 116,500 lbs. × 1,196 B.Th.U. =	139		Evaporating 996,000 lbs. water from 165° F. to steam at 1,196 B.Th.U. per lb. ... ... =	1,059
and heat sent to feed 996,000 × 90½° F. =	90		Loss to chimney	321
		49		1,380
Heat rejected to condenser by main engines by difference 879,500 lbs. × 1,123 (total heat to 100°) =	988			
Less work =	174			
		814		
Heat to chimney		321		
Lost in hotwell, 879,500 lbs. × 25½°		22		
		1,380 × 10^6		1,380 × 10^6

It appears that the actual net heat chargeable against the auxiliaries is the very modest figure of 48 × 10^6 B.Th.U. per hour, which is—

$$\frac{48}{1{,}380} = 3{\cdot}75 \text{ per cent. of the total heat.}$$

It is very evidently economical to treat the auxiliary exhaust in this manner on grounds of steam consumption alone.

A comparison of the economic results of these trials with the recent steam consumption tests at Carville brings out some interesting points which are best treated diagrammatically (page 112). The curves are plotted from the data published by Messrs. C. A. Parsons & Co., February 17, 1907. For direct comparison at relative fractions of full load the Carville figures are adjusted by translating the kilowatts into shaft horse-power assumed to be delivered by a motor of 94 per cent. efficiency—*i.e.*, 1 K.W. equals 1·26 S.H.P. The Carville trials were with superheated steam, and a correction of 1 per cent. for each 10 deg. Fahr. of superheat has been applied, so as to make the comparison under no superheat conditions. The vacuum at Carville was practically 29 in., as against 28·1 in. maximum on the *Lusitania*. The efficiencies are affected by this difference, but no correction has been applied. The superheat correction appears to be too much at full load. The overload results have been discarded. (The efficiency was maintained nearly constant up to double full load.) The shaft horse-power at Carville has been multiplied by 14 to bring it to the same scale of abscissæ. This diagram brings out the fact that an electric transmission of the main power on this ship appears possible with an improvement in

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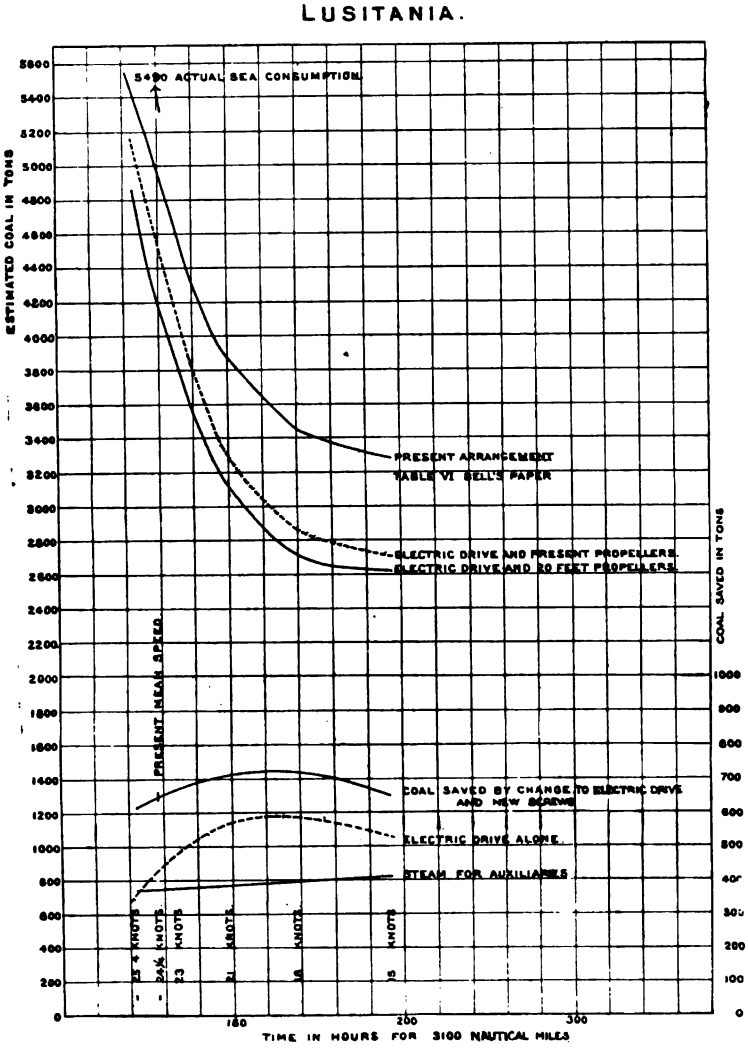
economy at full load, and a very considerable saving at less than full load. This comparison is shown in the diagram below in terms of total coal per trip, and an additional curve is drawn showing the possible saving by a reduction in propeller speed of revolution with increase in size, rendered possible by the use of an electric transmission. These figures are put forward only as suggestive of the opening for investigation, and not as a solution of the problem. On this diagram is spotted the actual sea consumption as resulting probably from increased resistance and resultant lowering of propeller and turbine efficiency at the lower speed. It is suggested that electric propulsion would minimise this lowering of efficiency.

DIAGRAM SHOWING COMPARISON BETWEEN COAL CONSUMPTION CALCULATED FOR EXISTING ARRANGEMENTS AND FOR ELECTRIC PROPULSION.


LUSITANIA.
5490 ACTUAL SEA CONSUMPTION
PRESENT ARRANGEMENT TABLE VI BELL'S PAPER
ELECTRIC DRIVE AND PRESENT PROPELLERS
ELECTRIC DRIVE AND 20 FEET PROPELLERS
COAL SAVED BY CHANGE TO ELECTRIC DRIVE AND NEW SCREWS
ELECTRIC DRIVE ALONE
STEAM FOR AUXILIARIES
PRESENT MEAN SPEED
ESTIMATED COAL IN TONS
COAL SAVED IN TONS
TIME IN HOURS FOR 3100 NAUTICAL MILES

Mr. THOMAS BELL (Member of Council): My Lord and Gentlemen, Mr. Parsons' reference to the propellers is certainly very appropriate; indeed, at present the *Lusitania* still carries her first set of propellers, and Mr. Luke is more hopeful than he led us to believe that improvements may be made on them. Of course, that matter rests with the Cunard Company. The ship has been shown to have attained her speed on trial, but I am quite sure the Cunard Company will not rest still they have still further improved upon it in that respect. Admiral Henderson's question regarding the consumption of coal is really largely replied to in the paper; that is, the consumption does not exceed what was expected. The consumption of the turbine per unit of power may not have thoroughly satisfied Mr. Parsons, but it has thoroughly satisfied us, and, I think, the Cunard Company also. Mr. Parsons certainly has reasons to hope for still better results, and I have no doubt that, if we, or others, have the privilege of constructing a second ship on this scale, we might attain that; but the consumption, I can assure Admiral Henderson, is thoroughly satisfactory, and the coal bunkers are ample for taking her across the Atlantic with a good margin to spare. They are made to hold 6,100 or 6,200 tons, so you see there is a very good margin—practically twenty hours' full-speed steaming left. I would also state that several gentlemen have very kindly referred to the amount of valuable information given to this Institution, and I think it is an omission on my part in not stating in the paper that I have to thank the managing directors of Messrs. John Brown & Co., Mr. Charles Ellis, and particularly Mr. John G. Dunlop, who is in charge at Clydebank, for free permission to avail myself of any information which they had there which might be of interest to the members of this Institution.

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A NEW SYSTEM OF SHIP CONSTRUCTION.

By J. W. ISHERWOOD, Esq., Member.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 9, 1908; Mr. S. J. P. THEARLE, Member of Council, in the Chair.]

OF late years considerable advance has been made in ship construction. Lower tiers of beams and decks have been dispensed with that were not long ago considered essential for structural strength. New types of vessels, such as the turret steamers of Messrs. Doxford, trunk steamers of Messrs. Ropner, and cantilever frame vessels of Messrs. Dixon, have also been built and proved to be successful, both structurally and commercially, yet the fundamental principles on which these and ordinary vessels are now being constructed are the same as were adopted in the earliest days of sea-going vessels.

So far all merchant vessels have been built with closely spaced transverse frames and closely spaced transverse beams, with the exception of one or two vessels built by Scott Russell in the sixties with longitudinal framing, full particulars of which will be found in Scott Russell's works. The *Great Eastern*, built on the box unit system, was of course the most notable example, and, being so well known, it is unnecessary to analyse the structure for the purposes of this paper. The difficulty and cost of erection were probably such as to prevent the system being generally adopted.

Naval architects have for so long recognised the great increase in strength obtained by framing a vessel longitudinally, that it is a matter for wonder that some practicable system of longitudinal framing has not been arrived at, or hitherto suggested, the advantages being so clear and so desirable.

In the new method of construction with which my name is associated, the closely spaced transverse frames and closely spaced transverse beams with which we are so familiar, are omitted. The transverse strength of the vessel is obtained by fitting directly on the plating a series of strong transverse girder frames and beams at widely spaced intervals, or where transverse bulkheads are fitted for the purpose of subdivision, as in oil steamers, for instance, they take the place of such transverses, but it is not essential that bulkheads should be fitted for obtaining the requisite structural strength. The transverse structures, which extend, where practicable, completely round the vessel, are of sufficient strength to withstand the whole

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collective water pressure on the skin of the vessel, and the upper portions forming the transverse beams are of such strength as to be able to carry the same collective weight on the deck as the greater number of transverse beams fitted in vessels of the ordinary construction.

It will be observed, from the diagrams, that the transverses are in all cases directly attached to the shell plating and deck of the vessel, and this is of the utmost importance as it enables the shell plating and deck plating, with the efficient longitudinal support which will be described later, to be considered as part of the transverse girder when calculating the comparative stresses.

The transverse thrust is also borne in mind when determining the arrangement and scantlings of the materials forming the transverse girder beams.

Having now briefly indicated how the main transverse strength of the vessel is provided, the arrangement of longitudinal stiffening is next to be considered. The shell plating and deck between the transverses are supported by continuous longitudinals, the transverses being slotted to admit of the longitudinals passing through them. The longitudinals are dealt with as girders having a length corresponding to the interval between the strong transverses, and are of strength not less than that of transverse frames of vessels of the ordinary construction, the comparative stresses being taken for each calculation under the same conditions as to water pressure, &c.

The longitudinal beams are similarly considered, but a deck load is taken as a basis; those at the uppermost deck are increased beyond the size required to give a corresponding stress on ordinary transverse beams on account of the longitudinals being also subject to longitudinal bending strains. Whether this increase is necessary or not in considering material of stiffening which is not already unduly stressed, and which at the same time is relieving the plating, is a matter for discussion.

In vessels with double bottoms, the stiffening, whether in the nature of plates, angles, or of sectional material, is also fitted longitudinally, but a transverse floor is fitted in line with the side transverses, and where the spacing of the transverses is great, an intermediate transverse floor, either intercostal or continuous, should be fitted in the bottom in order to provide for sufficent transverse strength for docking purposes and to efficiently support the longitudinal stiffening in the bottom.

It will be apparent that the arrangement of material provides for increased longitudinal strength, and admits of the shell plating and deck plating, when of substantial thickness, being reduced below the recognised standards of the various Classification Societies. It would appear that a vessel so constructed could safely withstand higher longitudinal stresses than a similar vessel framed in the ordinary manner by reason of all the plating being supported in the direction in which the greatest stresses occur. This consideration is in strong contrast with that obtaining in the ordinary transverse framed vessel, where the deck plating especially is nearly devoid of the desirable longitudinal support. The fact of many vessels having buckled

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their decks between the beams proves that more efficient longitudinal support is necessary than is fitted in ordinary cases.

The new system adapts itself to a rearrangement of the pillaring of ships in such a way as to overcome a cause of serious obstruction in the holds, pillars being only required at the strong transverse beams. To obtain equivalent results in the ordinary transverse system, it would be necessary to greatly increase the strength of every transverse beam and the connecting knees.

The structure is simplified, all parts are easily accessible, and maintenance repairs are in consequence reduced, a feature that should be welcome to owners and their superintendents.

A vessel so constructed would not be so liable to be broached by collision. It will be obvious that before the sides could be pierced it would be necessary for the longitudinals to be fractured, whereas in the ordinary transverse framed vessel there are only the side stringers to give fore and aft support to the plating between the transverse frames, and there is a growing tendency to dispense with this support, and to compensate for its omission by increasing the thickness of the plating and by fitting transverse frames of increased rigidity.

The question of facility for carrying out damage repairs is an interesting one in considering a new system of ship construction. In the system under consideration such damage as would be caused by grounding or collision could be repaired more expeditiously than in ordinary vessels. In the event of damage to the bottom longitudinals, it would only be necessary to remove the plates in way of damage, and the longitudinals are at once accessible without removing the plating across the bottom, as is necessary in a transverse framed vessel. Damage repairs through collision could also be effected with despatch on account of the simplification and accessibility of all parts of the structure.

It might be suggested that the deep transverses are a source of obstruction, but the obstruction is not so great as would appear to be the case from a casual glance at the sketch of midship section. In ordinary cargo vessels, the transverses could be fitted from 12 ft. to 20 ft. apart, and there are no broad horizontal plate stringers with bracket supports as in a web-framed ship, nor would it be necessary to fit widely spaced strong hold beams with broad plate stringers on the ends, such as are often fitted in deep single-deck ships framed in the usual manner.

It might here be explained, in regard to the spacing of the strong transverses in the new system, that the wider the spacing the heavier is the ship, owing to stronger longitudinals at the sides and deck being necessary. An economical spacing is found to be from 12 ft. to 16 ft., and there is practically no limit to the depth to which single-deck vessels, without hold beams, could be constructed. At the same time an important saving in weight of material, as compared with a transverse framed ship, would be effected.

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It will be observed that limbers can be reduced to a minimum in the new type of vessel, that part being almost recovered for either water ballast or cargo, and between the transverses the space is quite clear of beam knees and tank-side knees.

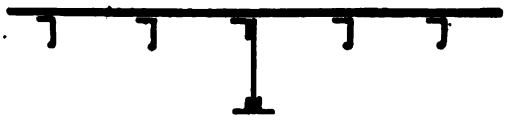
Dr. Bruhn discussed methods of comparing transverse strength in an able paper recently read before the Institution, and it would unduly prolong this paper to give and discuss all the calculations that have been made. For the present it will probably be sufficient to show the principal bases which have been worked to, and to state the result of these calculations, for determining the main scantlings of an oil-carrying steamer now being built by my firm, Messrs. R. Craggs & Sons, Ltd., Middlesbrough, for Messrs. Lennard's Carrying Company, Ltd., a firm who have had considerable experience in managing and repairing steamers engaged in carrying oil in bulk.

Fig. 1 (Plate X.) is a midship section of an ordinary transverse-framed vessel.

For purposes of comparison, a strip of plating corresponding to a frame space is considered as part of the frame girder. The stress is then ascertained by assuming a water load on the frame girder shown by the line drawn from the gunwale to a point A, the distance D corresponding to the depth of the lower part of the frame girder below the gunwale. For convenience in calculation a mean load is assumed.

The figures obtained, without assuming any internal support from the cargo, might reasonably be taken as the stresses the frames are subject to under ordinary working conditions, the head of water assumed at the water line corresponding to the freeboard, and being a fair allowance for the increased pressure due to waves, &c.; in any case, the figures need only be used in a comparative sense, although investigations in many cases show the calculated stresses to be reasonable and such as allow of a fair margin of safety.

In considering a vessel with web frames, a strip of shell plating corresponding to a web-frame space is taken as part of the frame girder along with the ordinary transverse frames on this strip of plating—

thus 

The load on the web frame is then taken over the web-frame space.

The results on this basis for web-frame ships generally show higher stresses than are found in the cases of vessels with either ordinary or deep frames.

For the beams comparison, one beam space of deck plating is taken as part of the beam girder and a load assumed on the deck, viz., 6 ft. high at 50 cub. ft. per ton for upper decks, and the actual height of 'tween decks at 50 cub. ft. per ton for lower decks.

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Fig. 2 (Plate X.) is a midship section showing the new system of framing. The strong transverse frames are considered as loaded in a similar manner to the ordinary transverse frames, a strip of plating being taken as part of the transverse frame girder. Although plating corresponding to the spacing of the transverses might reasonably be taken as part of the girder in the new system, as allowed in the transverse system, only half this amount of plating has been so considered in the calculations for these structures in the oil vessel referred to.

In this vessel the depth of the transverses has also been increased beyond the equivalent arrived at by comparison with either a web frame or a deep transverse frame on the above basis. This has been done in order to reduce the shearing stresses on the rivets attaching the transverses to the shell plating, and the obstruction is no detriment when liquid cargo is carried. The longitudinals are similarly considered, the length of girder being taken as the distance between the transverses, and the head of water corresponding to the distance each longitudinal D D is below the gunwale.

In addition to calculating the comparative stresses for ordinary working conditions in the oil vessel, as before mentioned, calculations were also made to ascertain the stresses under Lloyd's Register test for oil-carrying vessels, viz., with head of water 12 ft. above the deck. This is a severe test, and it is open to question whether it is not too great, possibly doing injury by subjecting the rivets to stresses which are much too high, having in view the duty to be taken in service.

The calculations for the deck beams, both transverse and longitudinal, are made in the same way, a strip of plating in each case being considered as part of the girder.

Messrs. Craggs & Sons and Messrs. Lennard's Carrying Company have kindly agreed to my publishing the principal scantlings and arrangements of this steamer, and the results of the principal calculations.

Fig. 3 (Plate X.) shows the midship section; A, in way of oil tanks; B, in way of machinery spaces.

Fig. 4. Longitudinal section, showing position of bulkheads and arrangement of transverse materials.

Fig. 5. Expansion plan, showing arrangement of framing.

Fig. 6. Expansion plan, showing plate edges.

Fig. 7 (Plate XI.). Details showing bracket connections of longitudinal bulb angle frames and bottom girders to transverse bulkheads, and also attachments of longitudinals to transverses.

Fig. 8. Arrangement of transverse bulkhead stiffening.

Fig. 9. B_1, B_2, and B_3, details showing bracket connections at the transverse and longitudinal bulkheads and (A) of the deck longitudinals to the transverse bulkheads.

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In addition to the vessel being constructed on the new system, the general design, for which Mr. E. Hall Craggs is responsible, differs in some respects from the ordinary oil-carrying vessel. The steamer is of the single-deck type, having a continuous expansion trunkway above the oil tanks; quadruple-expansion engines are to be fitted amidships, and arrangements have been made for three main boilers to be fitted abreast.

The coaling arrangements are very simple and very little trimming is necessary, the cross bunker forward of the boiler-room (A, in Fig. 4, Plate X.) being the main permanent bunker. The bridge space is to be utilised for reserve bunkers. A double bottom available for carrying oil fuel or water ballast is to be fitted in the machinery spaces.

In order to avoid any break in the longitudinal strength, the trunk is continued through the bridge, and it is interesting to note that the arrangements are such as to provide for practically the same longitudinal strength through the machinery spaces as in way of the oil tanks at each end of the bridge, the omission of the centre bulkhead being compensated for by the longitudinally stiffened bridge and double bottom. The tank abaft the engine-room is for carrying part cargo of refined spirit, and is fitted with a cofferdam at each end. The short tanks at the ends of the vessel (B, B, in Fig. 4) provide for taking a cargo of spirit when more capacity is required than for carrying heavy oils, or these tanks may, when required, be used as supplementary tanks for special uses.

The dimensions of the vessel are as follows:—Length b.p., 355 ft.; breadth, extreme, 49 ft. 5 in.; depth at centre, 29 ft. The scantlings have been approved by three Classification Societies, viz., Lloyd's Register of Shipping, Bureau Veritas, and British Corporation, and the vessel is being constructed for the highest classification of each.

Transverses.—The main oil tanks are 30 ft. long, and two transverses are fitted in each of these tanks. The transverses are 35 in. deep at the side, 20 in. across the deck, 39 in. at the bottom, and 33 in. at the middle line bulkhead. They are formed of plates $\frac{8}{20}$ in. in thickness and are connected to the shell plating by double angles. Double-face angles are fitted 5 in. × 4 in. × $\frac{10}{20}$ in. In the engine and boiler spaces, practically the same spacing of transverses is maintained. In way of the double bottom the alternate transverses are fitted continuously around the bottom to the middle line, and the longitudinal girders are fitted in long lengths between these transverses, to which they are efficiently attached. The remaining transverses are stopped at the deep girder in the double bottom next the margin plate, and are then fitted intercostally between the longitudinals to the centre line. The margin plate is fitted intercostally between the transverses and connected to them by double-riveted water-tight collars.

Longitudinals.—The uppermost bulb angle below the upper deck (No. 1, in Fig. 3) is 8 in. × 3½ in. × $\frac{8}{20}$ in. amidships, and is reduced to 7 in. × 3½ in. × $\frac{8}{20}$ in. at the

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ends; the lowest bulb angle (No. 9) is $9\frac{1}{2}$ in. × $3\frac{1}{2}$ in. × $\frac{12}{20}$ in., and the intermediate bulb angles are graduated in size according to the depth of immersion. They are spaced 29 in. apart. The bottom longitudinals are 15 in. × $\frac{8-7}{20}$ in., graduating in depth to 12 in. × $\frac{8-7}{20}$ in. at upper turn of bilge; the angles at top and bottom of these girders are $3\frac{1}{2}$ in. × $3\frac{1}{2}$ in. × $\frac{8}{20}$ in. The deck longitudinals are 7 in. × 3 in. by $\frac{8}{20}$ in. amidships, except in way of the bridge, where they are $5\frac{1}{2}$ in. × 3 in. × $\frac{8}{20}$ in., and they are also this size at the ends of the vessel. They are spaced about 27 in. apart. As previously mentioned, the longitudinal materials at the upper and lower parts of the structure for about the midship half length are increased in strength beyond that required to resist the water pressure, on account of these parts being also subject to bending strains. The trunk deck longitudinals at sides of hatchways are 7 in. × 3 in. × $\frac{7}{20}$ in. bulb angles; those in between the hatchways are reduced to $5\frac{1}{2}$ in.

The upper deck and trunk sides are $\frac{10}{20}$ in. thick, the upper deck stringer plate is $\frac{12-9}{20}$ in. clear of bridge and $\frac{10}{20}$ in. in the bridge. The stringer plate at the bridge ends is $\frac{16}{20}$ in. thick. The trunk stringer plate and trunk deck centre plate are $\frac{10}{20}$ in. thickness. The sheer strake is $\frac{12}{20}$ in. in way of the bridge, $\frac{16}{20}$ in. at the bridge ends, and $\frac{9}{20}$ in. at the ends of the vessel; the side plating is $\frac{11}{20}$ in. to $\frac{9}{20}$ in., and $\frac{12}{20}$ in. to $\frac{9}{20}$ in. alternately, and the bottom plating is $\frac{13}{20}$ in. amidships, $\frac{9}{20}$ in. in fore peak, and $\frac{10}{20}$ in. aft. The bridge side strakes of plating are $\frac{9}{20}$ in. and $\frac{8}{20}$ in. respectively. Three strakes of plating at each side of the keel plate have their midship thicknesses maintained to the collision bulkhead and the flat of bottom from No. 4 bulkhead (numbering from forward) to the collision bulkhead is additionally strengthened by the fitting of double 6 in. × 6 in. angles to the transverses and double angles $3\frac{1}{2}$ in. × $3\frac{1}{2}$ in. to the longitudinals.

From the expansion plans it will be observed that the plate edges up to the strake below the sheer strake are parallel to the keel, and that the spacing of the longitudinals at the ends of the vessel does not exceed the midship spacing, but is in some parts closer than amidships. The longitudinals are also so arranged that the crossing of the plate edges is almost avoided. The uppermost longitudinal on middle line bulkhead in trunk is 7 in. × 3 in. × $\frac{7}{20}$ in. bulb angle, and the bottom longitudinal is $9\frac{1}{2}$ in. × $3\frac{1}{2}$ in. × $\frac{11}{20}$ in., the intermediate stiffeners being graduated in size.

The transverse bulkheads (Fig. 8, Plate XI.) are supported on one side by three deep web plates A on each side of the middle line bulkhead and on the opposite side with horizontal stiffeners in line with the longitudinals on side plating and the longitudinals on middle line bulkhead. The sizes of these horizontal stiffeners are graduated according to their depth of immersion in a similar manner to those at the sides of the vessel, and on the middle line bulkhead.

The longitudinal frames and beams and longitudinal stiffeners on middle line bulkhead are cut at the transverse bulkheads, and are fitted with brackets efficiently connected to stiffeners and bulkheads in order to preserve the continuity of strength.

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Reference might here be made to the attachments fitted to connect the side stringer plates and bottom side keelsons in oil vessels built on the ordinary transverse system. Preparatory to determining the scantlings for the vessel described, investigations were made with a view of ascertaining the stresses these connections are subject to under test conditions. This was found to be a complex problem on account of the arrangement of materials adopted. The web frames with their attachments to the shell plating, as usually fitted, are not in themselves of sufficient strength to withstand the whole collective pressure, the intermediate transverse frames are not of sufficient strength to admit of the side stringers being omitted, and it would therefore appear as though the side stringers, acting as girders between the transverse bulkheads, did the bulk of the work in supporting the transverse frames without obtaining the requisite support from the web frames.

If the side stringers in an oil steamer with ordinary framing and web frames are considered as girders between the transverse bulkheads, the stresses at the ends of the girders are found to be abnormally high, and such would appear to be the explanation for the serious troubles experienced in so many of these steamers at the bracket attachments to the transverse bulkheads. The investigation points to the necessity of further increasing the depth of the web frames in ordinary oil-carrying vessels, and also of fitting double-riveted attachments to the shell plating as prescribed by the rules of Bureau Veritas.

It is of interest to note, in the new rules of the British Corporation Registry for oil carrying vessels, that this society has the courage to provide for "deep frame" construction in this type of vessel. Close investigation shows this to be distinctly preferable to the web framed arrangements generally adopted, and this is confirmed by the experience gained in steamers having deep frames in deep water ballast tanks.

In the system of framing under consideration, it is a simple matter to single out and determine the stresses on all parts of the structure under test conditions. Under these conditions the rivets attaching the brackets to the transverse bulkheads are subject to stresses in no case exceeding 3½ tons per square inch, which is only a fraction of that to which the stringer attachments in ordinary vessels are subject when considered under the same conditions. The strength of the rivet attachments in all parts of the structure in the new system have also been carefully investigated. The transverses are connected to the shell plating with double angles, having the rivets spaced about four and a half diameters apart. The rivets attaching the longitudinals to the shell plating are spaced six diameters apart, except in the vicinity of the transverses. The shearing stresses at these parts being at a maximum, a closer spacing has been adopted, viz., from three and a half diameters to four and a half diameters, except at the uppermost longitudinals where a uniform spacing of six diameters has been adopted.

It is a debatable point whether direct attachments of the longitudinals to the transverses are necessary, or whether the shell plating itself does not provide

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a gusset binding the two parts of the structure efficiently together. It would appear that if both longitudinals and transverses are efficiently riveted to the plating in the manner previously described no individual movement could take place.

In this oil-carrying steamer, however, and having in view the testing of tanks before the vessel is launched, it was decided to attach the bottom longitudinals to the transverses with vertical lugs having six rivets in each flange, the lower bulb angles with four rivets and the upper ones with two rivets. These attachments, whether structurally necessary or otherwise, were expected to facilitate the erection of the steamer, but in actual practice they proved of little importance. The transverses, where connected to the middle line bulkhead, are attached to the plating with single angles double riveted, and, as at the skin plating, the rivets are closer spaced in the horizontals in the vicinity of the transverses. The deep web plates on transverse bulkheads are attached with single angles having the rivets four and a half diameters apart and a uniform spacing of five diameters is adopted for the lower horizontals and six diameters for the upper ones.

The comparative longitudinal stresses calculated under the usually accepted loaded conditions, viz., by assuming the vessel on a wave of her own length and the bending moment as $\frac{D \times L}{35}$, show the tension at the bridge gunwale amidships to be 18½ per cent. less in the vessel built on the new system, the results being as follows :—

New system	6·7	tons per square inch.
Ordinary system	8·22	„ „

In making the comparison at the bridge ends, the bending moment was assumed $\frac{D \times L}{45}$ on account of these parts being some distance from amidships. (Whether the factor 45 is correct or otherwise is unimportant, since it would not materially alter the comparison.) Under these conditions the comparative stress at top of trunk is 10·8 per cent. less in the new system, the results being as follows :—

New system	6·61	tons per square inch.
Ordinary system	7·41	„ „

The comparative transverse and deck stresses on the bases previously described are shown in the table on the next page. Although this comparison of stresses shows the structure to be of generally increased strength, yet there is a saving in weight of material estimated at 275 tons. The saving is due to the redistribution of material, which admits of dispensing with a large number of transverse connections. The vessel, in consequence, could carry 275 tons more deadweight on the same draught, with the same model and with the same consumption of fuel, thus considerably increasing the earning power of the steamer.

A few remarks as to the erection of the ship may be of interest. The keel plate was laid and middle line keelson fitted as usual. The after peak is framed with the usual transverse frames, and the erection up to the peak bulkhead was the same as in

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Oil Tank Ship. *Transverse System.*	Tons per square inch.	Oil Tank Ship. *"Isherwood" System.*	Tons per square inch.
Deep Frames (increased as in deep tanks)	7·13	Transverses—Side	3·76
		Bottom ...	3·67
Web Frames (increased as is usual in oil steamers) ...	8·21	Deck	·96
Deck Beams (in bridge) ...	5·04	Deck Longitudinals—	
		In bridge ...	3·8
Deck Beams (clear of bridge, and increased as usual in oil steamers)	4·32	Clear of bridge	2·25
		Skin Longitudinals. Nos. 1 (Bulb Angles.)	1·17
		" 2 (Bulb Angles.)	2·34
		" 3 (Bulb Angles.)	3·5
		" 4 (Bulb Angles.)	4·49
		" 5 (Bulb Angles.)	4·38
		" 6 (Bulb Angles.)	4·65
		" 7 (Bulb Angles.)	4·92
		" 8 (Bulb Angles.)	5·46
		" 9 (Bulb Angles.)	5·89
		" 10 (Plates and Angles.)	4·02
		" 11 (Plates and Angles.)	4·34
		" 12 (Plates and Angles.)	3·97
		" 13 (Plates and Angles.)	3·62
		" 14 (Plates and Angles.)	3·31
		" 15 (Plates and Angles.)	—
		" 16 to 19 (Plates and Angles.)	3·31
Maximum shearing stresses on rivets attaching deep frames to shell plating ...	6·7	Maximum shearing stresses on rivets attaching strong transverses to shell plating	4·94
		Maximum shearing stresses on rivets attaching longitudinals to shell plating ...	1 to 5

ordinary vessels. The peak bulkhead and the two transverse bulkheads forming the two short compartments aft were then put together on the ground, erected and secured in position, middle line bulkhead fitted, and longitudinals placed in position.

The two transverses in the next compartment were erected on the port side of the ship, circular tunnel built in, middle line bulkhead fitted, and transverses on starboard side then erected. The longitudinals at bottom and sides were lifted into position, and the next transverse bulkhead erected, and so closed up that tank. The same method of procedure was repeated for the remainder of the vessel. The erection of the forward part, where clear of tunnel, showed the simplicity of the system.

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The brackets for the attachment of skin longitudinals to the transverse bulkheads were hydraulically riveted to the longitudinals, and the lugs on bulkheads to receive these brackets, horizontal stiffeners on transverse bulkheads and on middle line bulkhead were also hydraulically riveted to the various parts before they were taken down to the berth.

It will thus be seen that as the erection advanced forward the work was, tank by tank, left in a position ready for riveting up, all the materials required for the structure being in position, with the exception of the longitudinal beams, and as these did not delay progress they were followed up at a convenient interval.

With regard to ribanding, one stout rough riband each side was used at the top of the transverses for securing the same and one on the transverse beams, each side, for fairing purposes, and these were all that were found to be necessary. The longitudinals take the place of ribands and only require to be secured to the transverses and to the lugs on the transverse bulkheads, which were carefully templated, and no other fairing was required. No difficulty was found in the laying off of the vessel, and practically the whole of the framing of the structure was made to templates prepared in the drafting loft.

We did not have the assistance of the usual experienced frame erectors, and nearly all the erecting has been done by apprentices, the North-East Coast carpenters, unfortunately, being out on strike owing to a wages dispute.

The officials, workmen, and all concerned took a most lively interest in the work, and the predicted obstacles and difficulties of erection of a ship on the new system were found to be non-existent.

Fig. 10 (Plate X.) shows an arrangement of transverse materials in a single-decked cargo steamer, having poop, bridge, and topgallant forecastle. The spacing of transverses shown is 12 ft., except in way of the double bottom, where an intermediate transverse is shown. This transverse enables the longitudinal stiffening in the bottom to be of bulb angle section, thus making this part very easy of access.

Mr. J. W. Isherwood (Member): Since writing this paper, I have been informed by Mr. J. E. Scott that he built one or two vessels in the seventies in which longitudinal frames were fitted at the sides and bottom with transverse beams of the ordinary spacing at the decks. Transverse tie bars were fitted on the inner edges of longitudinals, and the transverse beams were simply bracketed to the sheer strake plating. It is unnecessary for the purpose of this paper to touch on the various systems that have from time to time been suggested which embrace the fitting of frames longitudinally, and I would suggest that the failure of Mr. Scott's system was due to the absence of provision for the necessary transverse strength, and to the method of framing the deck transversely.

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DISCUSSION.

Mr. J. Foster King (Member of Council): Mr. Chairman and Gentlemen, I did not expect to open this discussion, which might have been more appropriately commenced by a shipbuilder rather than by one of those who are committed to official approval of the structural arrangements of the design described by Mr. Isherwood. In the comparisons of the strengths of the component parts of the structure which are given in the paper, I assume that Mr. Isherwood did not intend that they should be accepted in any way as definitions either of first principles or of means for ascertaining the stresses which may be permitted on particular parts. I take it that the figures are purely comparative, and that, having stated their bases, Mr. Isherwood leaves us free to judge as to their quantitative value. There is, however, one comparison regarding which I think it right to say a few words—viz., the comparison between the longitudinal stresses on the bridge amidships and in the wells, and in which the bending moment in the wells is assumed to be $\frac{3}{4}\frac{5}{5}$ of that amidships. I do not know whether this is a practice which is customary with others, but I would like to take this opportunity of saying that, in my opinion at least, it would be dangerous to assume in ordinary cases that the strains at the fore end of the bridge are so reduced as to permit the use of smaller bending moments at that point. My own opinion—and I think the teachings of experience bear this out—is distinctly in favour of the contrary idea, that if the bridge finishes at the half-length, or anywhere within the midship half-length, and if the material in way of the bridge is that which experience and calculation have proved to be sufficient, the stress upon the upper deck material at the break of the bridge should be, if anything, lower than in way of the bridge, with the same or maximum bending moment. I take it that the figures in this particular comparison are due to the fact that the necessities of design cause a trunk to be fitted in the bridge and the bridge deck to be completely plated, or, in other words, that there is an excess of strength in the bridge, and not that there is a permissible difference in strength between the bridge and the well.

Mr. Isherwood, in his opening paragraph, refers to the remarkable advance during recent years in methods of ship construction, and makes the statement that "the fundamental principles on which these and ordinary vessels are now being constructed are the same as were adopted in the earliest days of sea-going vessels." The statement is correct, but probably its interpretation is wider than was in Mr. Isherwood's mind when he made it. There has certainly been a remarkable development in shipbuilding methods during recent years, and that development has been based on the earliest fundamental principles; but, without going back to ships which had no transverse framing, or labouring a point which perhaps I have made before, these principles include a return to and development of designs in which all the structural strength is contained in the external walls. The recent development includes a partial recovery from the retarding influence of the intermediate period when it was considered necessary to have a large number of decks in order to obtain strength. I think we may accept it that, technically, the web frame is the most scientific disposition of transverse material available to the shipbuilder; but, unfortunately, previous systems of web-frame construction were superimposed upon systems of framing designed for entirely different conditions, so that their full economic possibilities have not been realised in ordinary practice. Mr. Isherwood has not only the advantage of starting from a fresh platform, unfenced by precedent, and of concentrating all the transverse work upon wide-spaced transverse members of large size, instead of distributing it over a number of small frames, but he is absolved by the simplicity of his design from the confusion of mind and action which is fostered by the ordinary combinations of longitudinal and transverse material. There is also an undoubted advantage in the fact of all the distributing members, if I may call them so, being attached to the shell and deck plating longitudinally, so that the plating is removed from all probability of buckling under compressive stresses. I suppose that Mr. Isherwood's statement as to the saving of weight made in the particular vessel now being built, after having been loaded, as regards strength, with the ideas of no less than three Classification Societies, may be taken as being in some measure the practical expression of the technical advantages of the design.

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When it was my privilege last week to inspect the vessel now being built, I was particularly struck, not only with the appearance of efficiency given by the sizes and disposition of the material and the obvious adaptability of the design to oil tank vessels, but by the mechanical advantage mentioned in the paper, that so much of the complicated internal work of an oil steamer could apparently be finished and made thoroughly good in daylight, before proceeding with the plating of the deck, tanks, or shell. The value of Mr. Isherwood's departure in construction will not be limited to the utilisation of fore and aft distributing members, but will have the effect of bringing new thoughts to bear upon accepted methods of shipbuilding. If this new design shows that the whole of the transverse attachments, beam-knees, margin attachments, &c., to which we are accustomed, can be satisfactorily localised at intervals of 12 ft., then modifications of opinion as to many details must result, and further steps will be made in the direction of simplicity. It is not possible to foresee whether or not this design will be an absolute success in every detail, but at least all three Classification Societies have, I think, concentrated their minds upon its scantlings and practical construction, and, so far as their knowledge goes, everything possible has been done to avoid failure. Nothing on earth is free from sin of omission or commission, but there is every reasonable expectation that this ship will be a structural success, and whatever faults she may have, there can, I think, be no doubt as to the valuable technical qualities of the design.

Mr. R. Saxton White (Member): Mr. Chairman and Gentlemen, I think this Institution is to be congratulated upon having placed on its records particulars of a new departure in the system of ship construction, and if Mr. Isherwood's anticipations are fulfilled, this paper may possibly be of great interest in years to come. I consider the author a most fortunate man in being associated with a practical shipbuilder who is willing to recommend his clients to take up this new system, thus affording Mr. Isherwood an opportunity of developing his ideas. There is another point upon which I think Mr. Isherwood is to be congratulated, and that is in the fact that in his new departure he has apparently secured the sympathy of our great Classification Societies. Those of us who, in the course of our professional career, have had to approach one of these bodies with new proposals have not usually received such favourable consideration as Mr. Isherwood, as he appears to have been able to persuade the three principal Classification Societies to accept about 275 tons less steel than is required by their rules for building a certain ship of very moderate dimensions. Such favourable treatment is a welcome sign, and it gives us, I think, courage to go forward with fresh ideas to the great Classification Registries.

In the few remarks I propose to make, I do not wish in any way to cast doubts on the general design of this system of construction, but I think there are one or two points which call for consideration. I presume that the amount of steel that you put into a ship has something to do with her structural strength, and the problem which we all have to solve is that of building the cheapest ship to do certain work. On page 123 it is stated that the saving (275 tons) is due to the re-distribution of material which admits *of dispensing* with a large number of transverse connections, and that is the point to which I want to call your attention. Now, with all due deference to Mr. Isherwood and the Classification Societies, if the latter will only allow us the same latitude in construction scantlings as Mr. Isherwood, this great difference in steel would disappear. If you look for a moment at Fig. 2 (Plate X.), and take out of your mind's eye the "transverse," which is a very old friend under a new name, I consider that there is a very serious deficiency in strength. If it has been necessary in the past, with the ordinary system of construction, not only to fit floors on every frame, but also brackets on every frame outside the tank side plate, why dispense with them under this new system, as it certainly appears to me that between these transverses there is absolutely nothing to carry up the strength from the tank side plate to the upper structure but the shell plating alone? I grant you, the transverses are there spaced about 12 ft. or 14 ft. apart; but in between these transverses, as far as I understand the system of construction, there appears to be nothing connecting the upper structure of the ship with the tank side and double bottom. Considering the experience of the past, where Lloyd's have considered it necessary to call not only for single fastenings to the tank side plate, but, in many cases, double fastenings, on every frame, and also extra diamond plates; yet in this new design, by a so-called re-distribution of material,

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the whole of these brackets placed there to resist torsional stresses are taken out altogether. I hardly call that re-distribution of material.

There is one other point regarding these transverses for the ordinary ship, and that is that you will observe that these are supposed to be spaced 12 ft. or 14 ft. apart with intermediate transverses extending out to the bilge. That is all right as far as it goes towards supporting the double bottom, and, provided there is some connection up the ship's side from that intermediate transverse which stops at present on the tank side plate; but there is no attempt to utilise it in strengthening the ship's bilge and side with intermediate transverses, or large brackets, and so increase the connection between the upper and lower construction of the ship. You will further notice on the midship section that there is nothing to support the bottom plating between the main longitudinals and transverses except two bulb angle longitudinals. That is a space of at least 36 to 40 square feet, probably more, only supported by two bulb angles against the upward pressure of the water, to say nothing of the case of a vessel taking the ground during loading. It is not within my knowledge that Lloyd's have ever accepted such a construction of a double bottom in an ordinary steamer. Therefore, I contend that, if Lloyd's will extend the same generosity to the old-fashioned construction of vertical framing that they have shown in approving this design, the assumed saving of 275 tons would very largely disappear. There is one point in Mr. Isherwood's paper on the foot of page 117 which I do not quite understand. He says: "The wider the spacing"—referring to the transverses—"the heavier is the ship, owing to stronger longitudinals at the sides and deck being necessary." If that is correct, I presume the converse would equally apply, and that the closer the spacing the lighter is the ship. The only interpretation that I can put upon that paragraph is that Mr. Isherwood is unconsciously recommending the ordinary vertical frame construction, and, given equal strengths, I do not see how the old system can possibly be much, if at all, heavier than Mr. Isherwood's.

With regard to tank steamers, I think the new system also leaves a great deal to be desired, in that the brackets connecting the bulkheads to the longitudinals, although numerous, are not sufficiently strong or well connected to withstand not only the local strains, due to the "working" about the bulkhead and the shell plating, during the testing and general service of each compartment when carrying her oil cargo. They also have to provide the necessary connections for maintaining the longitudinal strength of the vessel as a whole at these points, and for this combined service, I consider, they are insufficient. Then, with all due deference to the Classification Societies, I consider the construction of the bottom distinctly weak. You have your transverses spaced 10 ft. or 12 ft. apart, you have one main longitudinal each side apparently supporting the transverses, and the whole of the upward pressure has to be carried by the longitudinals. The main transverses are 10 ft. apart, one longitudinal is a quarter of the beam, which may be taken as about 12 ft., so that there again you have a space of about 120 sq. ft. supported only by these longitudinals. Presumably, this has all been gone into, but this construction is, nevertheless, distinctly weaker than the present construction with floors of ordinary depth. I also consider the bracket supports at the foot of the centre line bulkhead inadequate to take up the work they will be called on to perform. Then, again, at the foot of page 122 Mr. Isherwood states that "it is a debatable point whether direct attachments of longitudinals to the transverses are necessary, or whether the shell plating itself does not provide a gusset binding the two parts of the structure efficiently together. It would appear that if both longitudinals and tranverses are efficiently riveted to the plating in the manner previously described, no individual movement could take place." I think that is entirely contrary to the practice of Lloyd's and the other Registries in the past. We have always been asked to connect whatever longitudinals we had with the transverse scantling by brackets or lugs, and if you take out all those connections and look to the shell plating alone to make up the connections between the transverses and the longitudinals, I contend that you are putting work on to the shell plating under this system which it is not called upon to fulfil to any appreciable extent in the ordinary system. There are other points having reference to the advantages of the system that I would like to refer to, had time permitted, but the ones I have dealt with require re-consideration before we could accept this as a satisfactory solution of a very interesting question. I hope

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that Mr. Isherwood will understand that the remarks I have made, and the objections which I have raised to the details of the construction, are not in the slightest degree antagonistic to progress, or to the proposed system of construction as a whole. On the contrary, I welcome the paper as being a step —it may be a very considerable one—in the right direction.

Professor J. J. WELCH, M.Sc. (Member): Mr. Chairman and Gentlemen, most of what I had to say has been covered by previous speakers; but, having had the advantage of seeing in the process of construction the ship here referred to, I shall be glad to make one or two remarks. In the first place, it seems to me that the longitudinal system of construction is a very useful one, and particularly so in cases where, as in the present instance, the ship being constructed is of a full form, so that it has something like 30 or 40 per cent. of parallel middle body. In that case you get long lengths of material in the direction of least curvature, and, as I think Mr. Foster King pointed out, one of the great advantages of this is that the stiffening is put in a position where it can resist the buckling stresses. With the present tendency to use high tensile steel, where the thickness is likely to be somewhat reduced, I think that is an important consideration. One cannot but be struck, on looking at the vessel under construction, with the fact that it does lend itself to easy erection; and a large portion of the work can be put together by hydraulic riveting, which I think we all admit to be an advantage. I take it that the real crux of the position is that which has been well pointed out by Mr. Saxton White. If, as a matter of fact, 275 tons of material can be saved by this method of construction, I think there is no doubt there is a very distinct future for it. Most careful calculations, we understand, have been made by Mr. Isherwood and the staff of Messrs. Craggs, and that is the figure at which they have arrived, and we can hope, therefore, that when the ship gets into actual service these calculations will be fully borne out.

Mr. J. L. TWADDELL (Member): Mr. Chairman and Gentlemen, it will be in the recollection of many gentlemen here who have had experience in the time of iron ships that, on the introduction of steel, consideration was given to the difference in material resulting to the extent, if I remember rightly, in a saving of something like 20 per cent. of weight. That was taken off on account of the difference in the homogeneity of the material, the strength of steel being greater than that of the material previously in use. To some extent, that would apply to a change in the distribution of material in a structure such as this. For instance, one feature in a vessel framed on the Isherwood system undoubtedly is the avoidance of so many rivets transversely throughout the ship, and I am sure that it is a good feature, provided the construction generally is not weakened against the pressure of water as Mr. Saxton White has suggested. Another feature is that for oil ships it undoubtedly has an advantage over the same system in ordinary merchant ships, because of the fact that, to begin with, in oil ships you have considerably closer spaced bulkheads, and therefore fewer of these special transverse frames in the Isherwood construction are necessary than would be the case in an ordinary vessel for merchant purposes. Another point is the fact that the sections can be better riveted by machine on the ground and assembled on the berth, thus securing much better work, especially by firms who have overhead facilities for handling material up to perhaps 4 or 5 tons in weight. We all know that in assembling the parts of a ship together, and also in the early stages of riveting, there is a great waste of time, much of which may be avoided or reduced by the introduction of some such system as this. There is the question, also, of carrying out repairs, where it becomes necessary to remove any of the transverse frames, and this system would undoubtedly be an advantage, seeing that, as the transverses are pierced for the longitudinals, it should not be a very difficult matter either to straighten a longitudinal or take one out and put another in without removing much of the shell, each shell plate having on it two of the longitudinals.

In the early part of the paper the writer says: "Naval architects have for so long recognised the great increase in strength obtained by framing a vessel longitudinally, that it is a matter for wonder that some practical system of longitudinal framing has not been arrived at, or hitherto suggested, the advantages being so clear and so desirable." With regard to that, in the Howden Yard of the Palmer Company, between thirty and forty years ago, a vessel of fair size for those days was constructed with her bottom framing entirely longitudinal and no transverses at all. Unfortunately, I have no means of knowing

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how far she was a success, either commercially or otherwise, but that is the fact. Then, about two years ago, the Palmer Company patented a system of framing which chiefly applied to decks in which longitudinals are the chief members—the Palmer-Long system, it is called. In this respect it is more longitudinal than the system now under discussion, because the transverses are the subsidiary and the longitudinals are the main members; while, I take it, from this paper and from the illustrations, the transverses are the chief members and the longitudinals are the subsidiary members in the Isherwood system. In this respect I am not quite satisfied, because I think, as Mr. Saxton White does, that the total surface that is suspended between these heavy transverses is probably not quite as efficiently stiffened against outside pressure as in an ordinary framed ship. Otherwise, there is much to be said for the system, and I am very much indebted to Messrs. Craggs' firm for giving me the privilege of seeing over this ship under construction a week or so ago. During that survey, I must say that it did not appear to me as if the saving of 275 tons were apparent, unless the shell plating (which was not on) was going to be very much thinner than we have to provide it in an ordinary framed ship.

Mr. J. E. Scott (Member): Mr. Chairman and Gentlemen, I am sure we have to congratulate ourselves on the able paper read by Mr. Isherwood in bringing this long-discussed subject before the Institution. The only point that I would bring before you is the fact that, as the last speaker pointed out, this is no new subject, as Mr. Isherwood would have us to understand from the title of his paper, and also by saying that naval architects had not given this their serious consideration long ago. I may tell you, Mr. Chairman, that I read a paper in this room in 1872, on the twenty-third of March, on exactly the same construction that Mr. Isherwood has put before you to-day—that is, with the longitudinal frames and the strong transverse tie-frames inside those longitudinals, spaced 10 ft. 6 in. to 12 ft. apart. In 1876 Messrs. Hanna, Donald & Wilson, of Paisley, constructed a vessel called the *Surabaya* on this principle. I myself constructed another vessel in 1877 on the same principle called the *King Koffee*. These two vessels, I may tell you, did their work most satisfactorily, so that all I have to say is that I hope Mr. Isherwood, having been so successful as to get an order for an oil tank vessel will push on with this longitudinal system of shipbuilding; but, as to putting it forward as a new and complete system that none of us knew anything about before, that is not the case.*

Mr. E. Hall Craggs (Member): Mr. Chairman and Gentlemen, I have been looking through the first few paragraphs of Mr. Isherwood's paper to try and find some of the matter which has been discussed, but I cannot find it. I do not see his claims to being the first to bring out longitudinal framing, neither can I see that he claims a purely longitudinal framing system. I do find, however—and those who had the kindness to follow the paper will at once recognise what I mean—that in the first place he gives the most prominent position to the transverse framing, and to those who have studied the subject of the framing of vessels it is impossible to deny that in endeavouring to use the longitudinal frames the easily traceable cause of failure has been the neglect of transverse strength. Having made these few remarks, I do not intend to follow the controversial matter any further—that is, as regards the denomination of the system. With Mr. Isherwood's kind permission, I have ventured to call this the "Isherwood system," and I do not think we claim any other name for it. The first point that Mr. Saxton White brings out in his remarks is the re-distribution of material, and he accuses us of simply leaving the stuff ashore. We do; we admit the soft impeachment; but he forgets that the only reason —or rather the only excuse—for repeating the transverse strength in ordinarily built vessels at short

* In answer to Mr. Isherwood's reply stating that my strong transverse tie frames were not continuous and had no beams, nor did they extend across the centre line, nor were they connected to the outside shell plating, I pointed out to the Chairman that this was a misstatement, and that in the letterpress of my paper read in 1872 it distinctly stated on the second page that inside the longitudinal frames those transverse vertical tie frames extended from upper deck stringer plates to upper deck stringer, passing continuously through lower decks and overlapped across centre line with beams at each deck having deep knees passing between the double angles of the tie frames, and connected to the skin or outside plating of the vessel by double angles as in my illustration. (Trans. I.N.A., Vol. XIII., Plate VIII.)

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intervals from stem to stern of the vessel, is that you cannot economically place it at greater intervals. The plating in between demands support, and there is no other way in the transverse system of getting it except by this repetition. Therefore, you must repeat these brackets that Mr. Saxton White refers to, and you must repeat all the connections. It is only when you turn to a system such as the one described here that you find, by simply looking fore and aft instead of across the ship, that those connections disappear—they simply are not there. You can put them in if you like, but you will see that you do not need them. You would not put a beam knee in the middle of a deck, neither would you put it in the middle of a longitudinal, half-way between the two transverses, so that we simply relieve the ship of a number of connections for which there is no longer any need. Again, by turning the skin frames round in a longitudinal direction, we at once condemn the stringers, as they have no function left. So much for the saving of weight. We do not profess to deal very particularly with the shell, but Mr. Isherwood has quite properly, in his calculations, recommended that for extreme proportions, the shell may be dealt with to some extent.

I am very sorry that Mr. Saxton White did not see the ship before he made his remarks, as I know he was very anxious to, but he had not the opportunity. Mr. Twaddell has seen the ship, and he denies that there is any *prima facie* probability of saving the weight claimed. He tells me the structure has a massive appearance, and certainly would not give rise to any misgivings as regards its ability to do the work required of it. As to the question of the supported area of plating, I do not quite agree with the previous speakers that that comes into the ship's calculations. The distance between the points of support seems to me of much more importance than the actual area of unsupported plating. You might run an unsupported strip of plating hundreds of feet if you had the two supports on either side near enough together to bridge the distance. As regards the means of persuasion we used to secure the co-operation and the valuable suggestions of the various Classification Societies, we did so by showing every one of our calculations, and we shall be prepared to show them to anyone who is sufficiently interested in the subject. Those who thoroughly understand such calculations will see at once how it is that the vessel is supported against all the strain she is likely to meet, and if you will take the trouble to run down the column of figures given in Mr. Isherwood's paper, you will see that she is supported much better than any transverse vessel of the same type and dimensions, built under the ordinary rule.

As regards the question of experience as against calculation, I would suggest that no amount of experience can ever justify unsound theory, and what is scientifically wrong can never be, in good practice, right. I will take one exa mple of this—in building oil vessels on the web frame system, unless you construct the webs sufficiently wide to hang your stringers upon them so that those stringers may in their turn support the intermediate frames, the whole weight of water on the stringer in that part of the vessel's side from bulkhead to bulkhead comes on your bulkhead brackets. I think that is obvious. I would suggest that we have gone much too far in increasing those bulkhead brackets in oil steamers, and if Mr. White had only made a simple arithmetical calculation he would have found that the webs were 4 or 5 in. too narrow, and he would have left the bulkhead brackets alone. Now, as to Mr. Scott's remarks, we are very pleased to have him here and to hear what he has to say. I must not say too much about his system, because I have not had the pleasure of seeing his ships, but it is obvious that no system of framing in which the frame is not riveted to the shell can get very far. I might add that no system of framing can get very far which frames the side of the ship and the bottom longitudinally and the decks entirely in the opposite direction, thereby making it impossible to provide any proper connection between the side framing and the deck framing.

Mr. H. M. Napier (Member): Mr. Chairman and Gentlemen, this has been an interesting paper of Mr. Isherwood's, and, having had many varieties of plans of oil-tank vessels through his hands, he must know all their weak points; but I quite agree with what has been said, that you can hardly call it a "new system." The *Great Eastern* and other vessels built by Scott Russell were on this longitudinal plan, and Mr. J. E. Scott has just pointed out that his vessel of 1872 was almost exactly the same. The great point that strikes one is that the author has got all three Registration Societies to accept and give their

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highest class to this vessel, which is 275 tons lighter than if built on the ordinary transverse system. As a shipbuilder, it strikes me that if all these Societies, in their united wisdom say that is the best system, giving maximum strength for minimum weight, the next thing will be that their classification rules will be changed and every ship will be built on this system. That is the logical conclusion. But there is one point that I do not quite see, namely, how the deck-supports (beams, &c.) in an ordinary trunked oil-tank vessel can be lighter if built on the longitudinal system. In such a vessel the distance from the side of the ship to the side of the trunk is not very great (about half the distance from one cross bulkhead to another), and I do not see how Mr. Isherwood can say that his system of strengthening that deck is lighter by taking the long way of the girder. The cross beam from the side of the ship to the trunk gives the shortest and, I should say, the lightest girder. Longitudinal strength is quite another affair, and no doubt the author's calculations of the stresses in that direction are quite correct. No doubt he will say that if he has the longitudinal system below he must carry it out above; but, as has been pointed out, vessels have already been built longitudinally below and athwartships above. I think, too, that the system is apt to make certain points almost too rigid and others too weak—for instance, the connections of the deck longitudinals to the bulkheads. In oil-tank vessels, as we know, it is the tightness of rivets that is most important, and if the vessel is very rigid in one place and flexible in another, you are very apt to have leakage. On the decks, too (you know how hot a deck is in tropical climates), it has struck me that with this longitudinal system there is bound to be a difference in the expansion between the upper surface—that is, the deck plating—and these longitudinal girders below the deck. A very little difference in length, due to difference of temperature, is bound to result in leakage.

Mr. J. W. Isherwood (Member): Mr. Chairman and Gentlemen, with regard to Mr. Foster King's remarks, I pointed out that, clear of the bridge, the factor of 45 was simply taken to use the results as a comparison. I never for a moment suggested that 45 was accurate; but still, I think that Mr. King is not quite right when he says that the stress clear of the bridge is greater than in way of the bridge, which, in the case of the oil vessel, is amidships.

Mr. Foster King: I did not say the stress was greater; I said the calculated stress would show a lower figure rather than a higher one on account of the change of section.

Mr. Isherwood: Of course, that is working on practically the same basis. We are all aware that as you work towards the ends of the vessel the bending moment is reduced, and I thought Mr. King had taken a wrong interpretation of my remarks. I quite agree we have had trouble in ordinary vessels at the bridge ends at the point where the bridge abruptly terminates. In this vessel there is the thick plating at the bridge ends, as in ordinary cases, but the calculation is made clear of that thick plating, and not immediately in way of it. I think Mr. King agrees with me in my statements that the fundamental principles at present adopted are the same, but what I particularly wish to lay stress on is that vessels have been built with closely spaced transverse frames and beams from the time of, say, Noah's Ark up to the present, with the exception of the one or two ships built by Mr. Scott Russell on a longitudinal system, and the two vessels that Mr. Scott has been good enough to speak about this afternoon.

In regard to Mr. Saxton White's remarks, I wish that my persuasive eloquence were as great as he makes out. If it were, I would try to prove to him that if you use transverse frames you must connect them at the tops and bottoms. An arrangement of transverse framing without these transverse connections of the frame girders would not be effective, and it is a difficult matter to understand why, because you must have these transverse connections in ordinary vessels, Mr. Saxton White suggests the fitting of them in the new type of vessel where there are no corresponding girders to connect with. With reference to the side stringers fitted in ordinary vessels, I cannot see how the attachments there can be omitted, as the stringers depend entirely upon these connections for being held in position. Mr. Saxton White does not appear to grasp the nature of the bottom stiffening in the ordinary cargo boat (the words "ordinary cargo boat" are here used to distinguish it from an oil-carrying vessel). Fig. 2 (Plate X.) shows midship section of an ordinary cargo boat on the new system, and Fig. 10 is a profile showing the arrangement of transverses. The transverses shown in Fig. 2 are drawn for a 12-ft. spacing. There is an

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intermediate transverse in the bottom, as shown in Fig. 10. The longitudinals are supported at 6-ft. intervals, and we therefore have to consider the longitudinals as girders only 6 ft. long, as compared, in an ordinary bottom, with girders having a length at least equal to the distance from the middle line to the side of the ship, which is greater by several times. I quite agree with Mr. Saxton White that Lloyd's have not had anything like it, nor has Mr. J. E. Scott even suggested anything like it. Referring to the bottom longitudinals in the oil vessel, of which full particulars are given in the paper, we have there to consider girders 10 ft. long as compared with girders (floors) in the ordinary transverse ship having lengths equal to half the breadth of the ship where a middle line bulkhead is fitted. I think a little investigation will show that this bottom is quite as strong as the bottoms of oil vessels which are now built. Mr. White points out that the shell plating in a vessel built on the ordinary transverse system is not called upon to do so much work as in the new system. In my opinion, it is to be regretted that the shell plating in an ordinary oil-carrying vessel has not been called on to do more work by giving it (the shell plating) more efficient attachments to the web frames. Further, had the web frames been of greater depth, there would not, I believe, have been anything like the trouble with the side stringer attachments to the bulkheads that has been experienced in the past, and that is occurring even to-day. To my mind, the remedy for these troubles is obvious, and that is to make the shell do more work, increase the depth of the web frames, and, if desired, reduce the width of the side stringers that are causing so much trouble at present, and thereby relieve the stresses on their bulkhead attachments.

Mr. Twaddell draws attention to the saving in weight, and it seems to me that he rather infers that we are reducing the substantiality of the parts to effect a saving in weight of material. Such is not the case, however, the materials in the oil vessel being quite as substantial, on the whole, or probably more so, than if built on the ordinary system, and the saving is simply due to the redistribution of the material, which admits of the omission of many subsidiary parts. I might here be allowed to answer Mr. Napier's question respecting the stiffening of the deck. In the new system the longitudinal girder is, in effect, the short girder, not the transverse girder.

Mr. NAPIER: No.

Mr. ISHERWOOD: In the oil vessel the longitudinal girder is supported at intervals of 10 ft. The length of the transverse girder would be the distance from the trunk side to the side of the ship.

Mr. NAPIER: Taking the whole of them, the transverses and longitudinals, in your system.

Mr. ISHERWOOD: Whether you take the whole of them or whether you take individual ones for comparison does not make any difference. You can deal with each member on its own account if you wish, or you can deal with them altogether. The transverse beam in the oil vessel shown in Fig. 3 (Plate X.) would be about 12 ft. 6 in. from the side of the trunk to the side of the ship, the corresponding length of fore and aft span is 10 ft. It will thus be seen that advantage is obtained from the short length of girder.

Mr. NAPIER: The transverses also.

Mr. ISHERWOOD: The transverses would be about 12 ft. 6 in. long, as compared with the supports to the longitudinals giving 10-ft. spans.

Mr. SCOTT: I thought your transverses were 12 ft. 6 in. apart.

Mr. ISHERWOOD: It is clearly pointed out in the paper that in the oil ship they are 10 ft. apart. I might also explain that the longitudinals at the deck are increased beyond the size that would give equivalent comparative stresses in view of the longitudinal bending they assist in resisting. I was pleased to hear Mr. Twaddell's remarks in respect of the steamer built at the Howden yard. He states the bottom was entirely longitudinal. This particular system, however, I do not claim to be entirely longitudinal, and should very much value Mr. Foster King's remarks on some future occasion about the construction of a bottom which is entirely longitudinal. With respect to Mr. Scott's system, which he devised in 1870, as Mr. Napier points out, the ship was built athwartships up above and longitudinally below.

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Mr. Scott: On the deck only.

Mr. Isherwood: Athwartships above and longitudinally below. The ship which Mr. Scott built—at least, I understood so from him—had two decks. Those decks had transverse beams 3 ft. 4 in. apart; there were fore-and-aft Z's or channels in the sides and at the bottom; inside these fore-and-aft Z's there were either T iron or double angle transverse ties. Those transverse ties did not extend round the ship.

Mr. Scott: Entirely round the ship.

Mr. Isherwood: The transverse ties did not extend round the ship; they extended from the middle line to the top deck, and were fitted in between the transverse beams. It must be perfectly obvious that to obtain equivalent transverse strength when you remove the transverse frames from the shell plating and fit them on the inner edges of longitudinals, you must of necessity have considerably more weight of transverse material to get anything like equivalent transverse strength.

Mr. Scott: But the transverse frames are continued from girder to girder.

Mr. Isherwood: Exactly. The transverse tie frames extend from the middle line up to the top deck, and are fitted in between the transverse beams.

Mr. Scott: Nothing of the kind; they are fitted from girder to girder, and form a complete frame (*producing copy of Plate VIII., Vol. XIII. Trans. I.N.A.*).

The Chairman (Mr. S. J. P. Thearle): The drawing shows the transverse frames to be between the beams and not attached to the beams.*

Mr. Scott: No; they are attached to the beams, and the specification describes it so.

Mr. Isherwood: I will read from your own specification: "This invention has for its object the building of iron, steel, or composite ships in an improved manner, so that such ships, particularly when of considerable or excessive length as compared with their breadth or depth, may be stronger for a given weight of material than has hitherto been constructed. The invention consists principally in the case of iron and steel ships in fixing the outside plating arranged longitudinally as at present upon longitudinal frames instead of upon transverse vertical frames"—note, it clearly reads, "instead of upon transverse vertical frames." "At the sides, these longitudinals may be formed of iron of Z section or of reversely united pairs of ordinary angle iron."

I leave it for your consideration, Gentlemen, could you build a ship of any depth having ordinary angle iron transverse frames which did not extend round the ship, and were fitted on the inner edges of the longitudinals, and were not directly attached to the shell? Such a ship, if of anything like the size of the vessel described in my paper, would surely drop to pieces.

The Chairman: Gentlemen, we have listened to an excellent paper, and we must congratulate the Institution on having had a very interesting and enlightening discussion upon it. I am sure you will join with me in conveying to Mr. Isherwood our best thanks for the very excellent paper that he has contributed to this meeting.

Written Contributions to the Discussion.

Mr. J. Bruhn, D.Sc. (Member): I have always been very much interested in the general question of efficiency in ship construction, and I have therefore had occasion to investigate pretty closely the merits of the various systems of longitudinal framing. I think Mr. Isherwood dismisses rather summarily what has been done before in the direction of longitudinal construction. The first vessel constructed

* Mr. J. E. Scott has since called my attention to that part of the letterpress of the paper he read in 1872 which he now quotes in the footnote to his remarks on page 130.—S. J. P. T.

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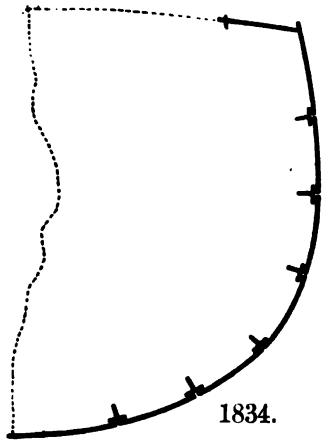
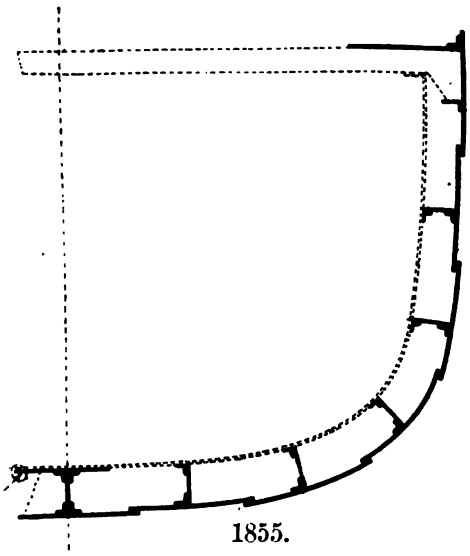
on this principle was probably the S.S. *Storm*, a steamer over 70 ft. in length, stated by Scott Russell to have been built by him in 1834 without a single transverse frame from end to end. In that case the distances between the bulkheads were small, and the stiffening to the shell plating consisted of longitudinal T bars acting at the same time as edge connections. This was a construction showing considerable originality on the part of the designer, and it would be quite an efficient one where the spacing of the bulkheads is so small. In 1844–1848 Mr. Scott Russell built two vessels with longitudinal framing in their midship parts. In the fifties quite a number of steamers were constructed on this principle. In 1855–1856 Scott Russell built, amongst others, the S.S. *Baron Osy*, the S.S. *El Rey Jaymes I.* and *Der Preusse*, all of which were classed in Lloyd's Register. They had continuous fore-and-aft plate stringers and intercostal transverse web frames of the same depth in their midship portions, but the beams were fitted transversely, as shown by the sketch.

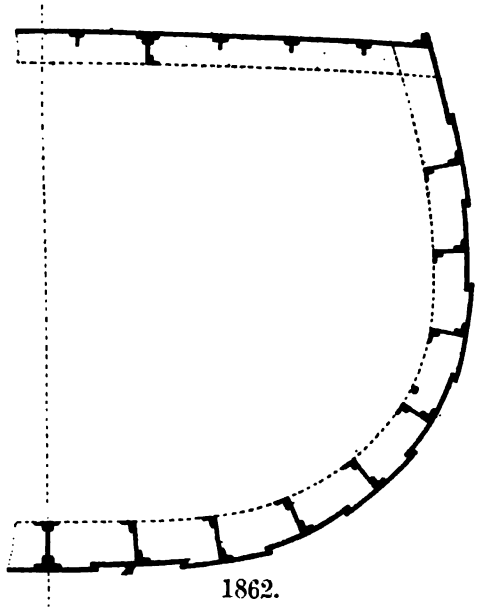


1834.

In 1862 Scott Russell built the *Annette*, which was also classed in Lloyd's Register, and was constructed on the same principle as the above-mentioned vessels, except that a system of deep transverse plate beams was introduced with fore-and-aft T bar beams. At this time vessels were also being built on the longitudinal principle in Danzig by a Mr. Jensen, one of Scott Russell's assistants. A superficial examination



1855.



1862.

of the systems adopted in these vessels will show that the uniform depth of the transverse and longitudinal girders makes these vessels relatively weak transversely. In 1867 Messrs. Randolph Elder & Company built two shallow-draught tugs for the Godavery River, a description of which will be found in the 1867 Transactions.* The construction of these vessels was exceedingly elegant. They had a thin flat bottom and a thinner deck, both stiffened by longitudinal angles and kept apart by longitudinal and transverse lattice work, which gave great strength with very little material. Mr. Scott Russell admitted the system was better than his own, and here, if anywhere, the longitudinal method of construction is the ideal one. In 1872 Mr. J. E. Scott proposed to fit continuous close-spaced fore-and-aft girders with double-angle transverse stringers having intercostal plates between the floors. This longitudinal system would be less efficient than the earlier ones, as it was even more deficient in transverse strength than these. In

* Trans. I.N.A., Vol. VIII. p. 1.

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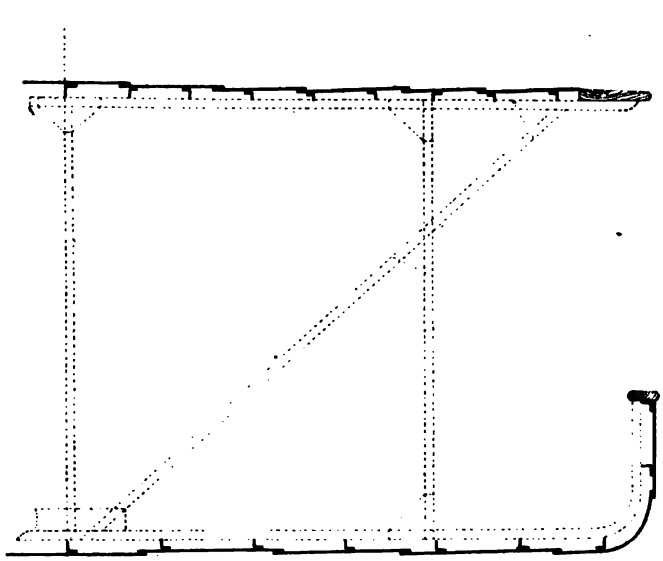
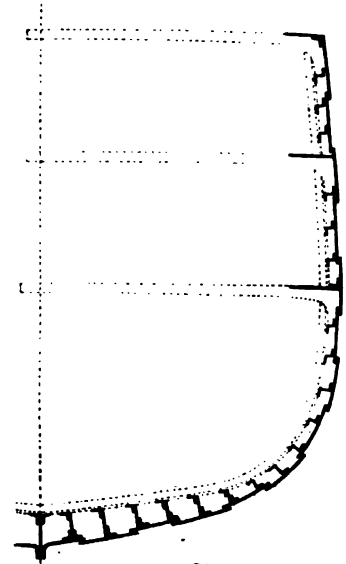
the eighties a sort of a cross between Mr. Scott's and Mr. Scott Russell's systems was proposed, with bulb angle fore-and-aft frames between a few deep longitudinal plate girders, and I believe a Swedish gentleman, Mr. Lilljehook, has taken out a patent on such an arrangement with the omission of the longitudinal plate girders. So far, the structural objection to all the longitudinal systems to my



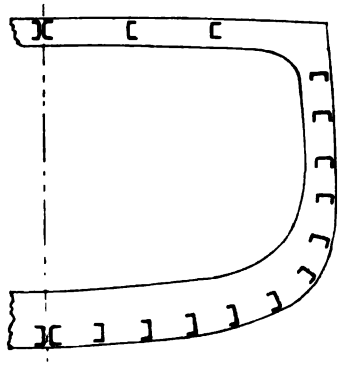
1867.



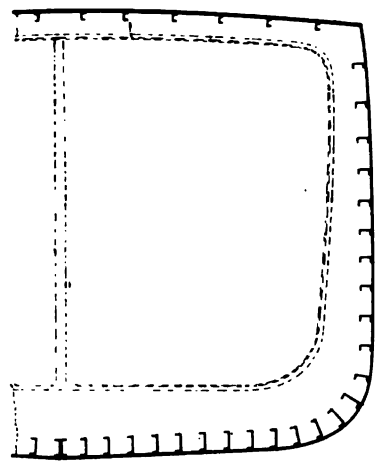
1872.

knowledge adopted or suggested for sea-going vessels had been the relative deficiency in transverse strength or the excess of longitudinal strength.

In a paper read before the Graduate Section of the Institution of Engineers and Shipbuilders of Scotland in 1897, I incidentally suggested a method of longitudinal framing where any desired amount of transverse strength could be economically obtained. This was illustrated at the meetings by a large-sized sketch, properly drawn, but for the sake of exactness I reproduce (marked 1897) the actual sketch published with the printed paper, which was made from a rough drawing, not to scale. In the text I stated that: "For strength alone the most economical distribution of the material could probably be arrived at by fitting small or local continuous longitudinal girders on the shell and deck plating and larger girders or partial or complete bulkheads close enough to prevent the smaller girders giving way between them. . . . Although this system gives great economy of material, as each part of the structure can be worked to its full capabilities, it is in most cases impracticable in the merchant navy, as the transverse bulkheads interfere with the storage of cargo, and the difficulties of construction are great." I looked upon the system as a sort of theoretical standard or ideal distribution of material, and I used it in that way in a paper I read before the Glasgow University students on February 1, 1906, before I knew Mr. Isherwood was working with this idea. A facsimile of the sketch from which the lantern slide then used was produced,



1897.



1906.

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is here shown (marked 1906). The conclusions I there came to were, briefly: That ships of sufficient strength could always be built with that arrangement of framing; that the system would be relatively more economical the fewer the decks that were fitted; that it would be most economical in single-deck and single-bottom vessels with no restrictions at all as regards encroachment on hold space, and that, even in these cases, very deep webs would have to be adopted with a spacing considerably less than the depth of the vessel, if the construction was to be lighter than an efficiently designed deep frame arrangement. It was also pointed out that the economical advantages the system possessed over ordinary transverse deep framing was not so much due to the girders being fitted longitudinally as to the adoption of deep and thin webs—a feature which could equally well be adopted with many transverse arrangements of framing. It is almost impossible to examine the strength of the various transverse systems of webs and deep framing without being driven to the consideration of this system of longitudinal framing. Mr. Isherwood has considered the advantages sufficiently great to give it a trial in practice, and I hope he will succeed. In one respect, at any rate, it is bound to be a great boon to all future naval architects, as the introduction of such radical changes in the method of construction are sure to add to our knowledge as to what is essential in a ship's structure, and to hasten the pace at which inessential features disappear. With regard to the lists of stresses given on page 124, I am afraid I must say that they do not quite agree with my estimate; but, then, the strength of a ship is not based entirely on moment of inertia calculations. Where actual scantlings are concerned it is necessary to look at the question in the broader light of experience, and to be very careful, particularly in respect to riveted attachments, where such radical changes are made in the dispositions of the material. I am sorry Mr. Isherwood has not given us a comparison between a vessel fitted with longitudinal frames and one designed in accordance with the other patented method of his firm, which I see described elsewhere as the last word in transverse ship construction. It is between these two systems that the fight for superiority must be fought, and I am rather inclined to think that the latter will have the last word; but thus, whichever wins, Mr. Isherwood will be the gainer.

Mr. William Gray (Member): The value of the information Mr. Isherwood gives in his paper regarding the vessel now being built on his system at Messrs. Craggs' would be greatly increased if he would give the details of the comparison of weights with those of an ordinary steamer of the same size. He gives the total saving as 275 tons, and he could, no doubt, without making an undue call on his valuable time, give in small compass some further explanation of one of the most remarkable features of his new system. The reduction in first cost of the vessel and the increase in the revenue-earning of such a large weight in a vessel of this size must be considerable. It does not appear from the description or the diagrams (i) whether the sheer strake is only connected by the stringer angle to the stringer between the transverses; (ii) whether there is any connection, except the tank margin angle, between the margin plate and the shell plating between the transverses.

Mr. J. W. Isherwood sent the following written reply:—Dr. Bruhn's contribution is of interest since it collects the various systems of longitudinal framing that have hitherto been attempted, and also the various systems that have been suggested, but nearly all the matter he gives has already appeared in the Transactions, and has received full discussion.

It is well known that several vessels have been built with longitudinal frames, and two or three with longitudinal beams also, but the reasons why the systems have not been commonly adopted are that in these designs clear provision has not been made for the requisite transverse strength in vessels of whatever dimensions, and the recognised difficulties of erecting such structures. It might be as well to add here that there is no novelty in recent years simply in the fitting of longitudinal frames or longitudinal beams.

Mr. Craggs has already pointed out that the "Isherwood" system cannot be strictly designated longitudinal. It would be more correct to state that it is a system of framing which readily admits of longitudinal frames and beams advantageously forming part of the hull structure, and by means of the system there is now ample proof that the difficulties have been overcome of building vessels with efficient and direct longitudinal stiffening as readily and economically as vessels built on the transverse system.

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THE HEATING OF MODERN OCEAN LINERS.

By W. CARLILE WALLACE, Esq., Member.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 9, 1908; S. J. P. THEARLE, Esq., Member of Council, in the Chair.]

THE subject of this paper can hardly be said to come under the heading of Naval Architecture in the strictest sense, and on this account I feel I owe the Institution an apology for bringing it before them. On the other hand, when we consider that the modern ocean liner has developed into a vast floating hotel, carrying many thousand souls, the importance, from a sanitary standpoint, of effectually ventilating every part of the vessel, and maintaining the passenger accommodation at an equitable temperature can hardly be overrated.

For vessels of even moderate size, some form of mechanical ventilation has proved to be a necessity, as only by that means is it possible to insure a rapid change of air throughout the passenger and crew accommodation in all kinds of weather. On the modern ocean liner, the higher priced staterooms and suites are occupied much more during the day than was the case when a stateroom was only a 6 ft. by 7 ft. box, with two or four berths in it, as the case might be. Passengers are becoming more difficult to please, and competition is becoming keener, so that a shipping company who wish to retain their patrons must equip their vessels with all the luxuries of a modern hotel. As maintaining the individual rooms at an equitable temperature is not the least important of these, I propose to restrict the scope of this paper more especially to this problem.

Among the first ocean steamship companies to give the question of ventilating and heating the attention it deserves, the American Line are well to the front, as the *St. Louis* and *St. Paul*, built in 1895, were equipped with a very complete system, consisting of a number of pressure fans, placed on the boat deck, forcing air by means of suitable ducts into the principal staterooms, public rooms, and alleyways, the air being warmed by passing over steam-heated coils, suitably arranged close to the fan discharge, suction fans being also provided and connected to another set of ducts, for drawing away the vitiated air from the different rooms. This system has

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worked fairly satisfactorily as far as the ventilation is concerned, but it has been found impossible to regulate the heat in the staterooms with any degree of certainty.

Leading a double set of ducts throughout the vessel is expensive in first cost, and, as space is naturally restricted, the ducts require to be kept small; this necessitates an increased velocity of air in the ducts so as to get sufficiently rapid change of air in the rooms, as the power to drive the fans increases as the square of the velocity, and it becomes apparent that this, together with the increased skin friction consequent on the higher speed of the air, very greatly increases the cost of the operation.

A very similar system of ventilation is fitted in the newer vessels of the Red Star Line, the warm air being discharged, in these vessels, into the public rooms and alleyways on the different decks, exhaust fans for the removal of the vitiated air being fitted, having ducts leading from the staterooms, these latter being dependent for heat on the warm air from the alleyways being drawn in through venetian panels in the doors to supply the place of the vitiated air drawn out by the exhaust fans. In cold weather, it is found that this system is inadequate to warm the outside rooms even when the temperature in the alleyways and other parts of the ship is high and the inside rooms much too warm. In summer, with the outside temperature uncomfortably high, this system is worse than useless as a means of cooling the vessel, as observation has shown that the air discharged into the alleyways, &c., by the pressure fans, even when no steam is passing through the heating coils, is, for several easily explained reasons, from 5 to 10 degs. Fahr. above the temperature of the outside air, and, as the exhaust fans alone are not of sufficient capacity to ventilate the vessel, there is no alternative in warm but stormy weather between having the vessel uncomfortably hot through forcing in fresh air, or stuffy and ill ventilated through depending entirely on the exhaust system.

To get over these difficulties numbers of other schemes have been tried, as, for instance, making the ventilation and heating systems more or less independent of each other, the heat in these cases being supplied by individual steam or electric heaters placed in each stateroom and more or less under the control of the passengers themselves, the ventilation being effected by exhausting the vitiated air from the rooms and alleyways, the fans and ducts being of sufficient capacity for this purpose, the fresh air entering by storm-proof ventilators, doors or other openings in the passenger accommodation. This system has the advantage of enabling the heat to be regulated without affecting the ventilation in the staterooms, the temperature there being adjusted to suit the varied ideas of the occupants. That this adjustment is a matter of no small importance will be vouched for by any chief engineer in the Atlantic service; for, should he be so unfortunate as to have Americans and Englishmen in staterooms supplied from the same warm air ducts, he will have a condition of things which will give him considerably more worry during the voyage than will the main engines.

There are so many manifest objections to placing steam heaters in all the rooms, that it is hardly necessary to enumerate them. They are apt to cause an unpleasant

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smell when first turned on, the multiplication of valves and pipes is a very serious evil, to say nothing of the risk of some of the heaters freezing and bursting while the vessel is lying in port in winter on the American side.

On the other hand, electricity lends itself admirably to individual heating at very small initial expense, as every stateroom is wired for lighting, and it only becomes necessary to increase the size of the wires to provide for the extra current. The only objection to its extended use hitherto has been the cost of operation.

With the idea of meeting the difficulties already enumerated, and dispensing, if possible, with individual heating, there has been installed on board the *Lusitania* and the *Mauretania* an elaborate system of heating and ventilation which is fully described and illustrated in the *Lusitania* number of *Engineering* (August 2, 1907), and the *Mauretania* number (November 8, 1907). All this work was carried out by the Thermotank Ventilating Company, Glasgow, and is, I think, without doubt the best and most thorough scheme so far supplied to any vessel. One special feature which, in the opinion of the writer, will be found of great advantage in warm weather, is the possibility of being able to reverse the direction of the air currents in the ducts, the fans under these conditions exhausting the foul air from the rooms in large quantities, thus causing fresh air to be drawn in through open side lights, doors, windows ventilators, and other available openings.

With regard to the practical working of the system, I believe it has given excellent results as far as the heating and ventilation of the third-class accommodation is concerned, also in the large public rooms and open spaces in the passenger accommodation. Unfortunately, from structural and other reasons, it has been found impossible to avoid supplying inside and outside staterooms with warm air from the same thermotanks; on that account, I fail to see how it is possible to supply a sufficient quantity of fresh air at the same temperature to ensure proper ventilation to both these classes of rooms, at the same time keeping them at the same temperature.*

It is only necessary to glance at the plan of the engine and boiler space of these vessels to appreciate the enormous amount of heat which must be radiated from surfaces inside the hull. The major portion of this heat, no doubt, escapes by means of the funnel-hatches, and engine-room and stokehold upcast ventilators, but still a large amount must be conducted through the decks, bulkheads, casings, &c., tending to warm up the interior of the vessel.

For this reason there must be many inside rooms even as high up as the promenade deck, which will require but a very small amount of additional heat to make them

* Since this paper was written, several additional thermotanks have, I believe, been fitted in the above vessels, the inside and outside staterooms being, as far as possible, supplied with air from different tanks. This should greatly assist in the maintenance of a more equal temperature in all the rooms, but it does not solve the difficulty consequent upon passengers, in adjoining rooms, having different ideas of what is a comfortable temperature.

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comfortable even in the coldest weather, whereas, for the outside rooms on the same deck, with one side, and in some cases two sides, exposed to the weather, a large amount of heat will be required. That this internal heat, so to speak, exists in a vessel, is borne out by observation, under all conditions of weather, conducted on the S.S. *St. Paul*, which show that unoccupied staterooms, depending on their position on the vessel, maintained a certain definite temperature above the outside air throughout the voyage.

From a careful analysis of the different systems of heating, I am led to the conclusion that the one best adapted to large passenger steamers is a combination of a system similar to that fitted on board the *Lusitania* and *Mauretania*, supplemented by a system of individual electrical heating, the whole combination being under automatic control.

Until very recently there has been no reliable automatic device for regulating the temperature of a room warmed by an electric heater. Numbers of electrical thermostats have been patented, but they have all failed under the test of practical application, and, whatever possible success they may have attained on land, they were absolutely useless on board a steamship owing to their extreme sensitiveness to vibration.

Some years ago the attention of Dr. Geissenger, surgeon of the S.S. *St. Paul*, was drawn to the necessity of some means of regulating the temperature in his stateroom, which was supplied with an electric heater, as he found that if he left the heater on while absent, the room became much too hot in moderate weather, whereas, if turned off, the room became too cold. Any attempt to use existing thermostats was an utter failure, as he soon found that, owing to vibration, the switch controlling the heater would be destroyed in a few hours, and the contact points of the thermostat itself ruined through oxidisation. As a final result of his investigations, he has succeeded in constructing an electrical thermostat which is positive in its action, controls the temperature within one degree Fahr., is absolutely unaffected by vibration, and requires only 2·75 watts for the controlling current.

For use in staterooms, the instrument is a combination of two thermostats in one case, the one set for a day temperature of, say, 68° Fahr., the other for a night temperature of, say, 64° Fahr., both these temperatures being easily adjustable to suit the requirements of the occupant of the room.

The use of a lower night temperature is very desirable from a health point of view, but it has also an important bearing on the quantity of current necessary to maintain this lower temperature as will be seen later. To change from the one temperature to the other, it is only necessary to throw over an electric switch, when the thermostat takes care of the rest. This thermostat has now been in use in Dr. Geissenger's stateroom for two years with perfect success.

That there is a very important saving in the amount of heating current when properly regulated, and that in such case the cost of electrical heating is moderate, is clearly demonstrated by the following report of and deductions from a series of

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observations taken during several voyages of the *St. Paul* and *Oceanic* between November 23, 1907, and January 25, 1908.

The surgeon's cabin is an outside room situated on the first covered deck, and is 7 ft. 6 in. by 8 ft. and 7 ft. 6 in. high. The ceiling of the room is composed of the bare plating and beams of an exposed deck. The outside panelling is entirely uncovered between two frames, and, therefore, this area is practically exposed to radiation through the steel hull to the external air.

The cabin is provided with a liberal supply of fresh air through a duct and 4 in. by 4 in. uptake, and the door is arranged with the customary venetian panel. The ventilation, therefore, is quite up to the standard, and for certain reasons the incoming air is rarely or but slightly heated. For this particular room the demand on the heat supply is therefore certainly as heavy as that of the ordinary promenade deck stateroom. The room is warmed by one of the regular electric heaters manufactured by the Consolidated Car Heating Company, of New York City, which consumes eight ampères at a pressure of 110 volts.

For the purposes of observation the days were divided into two periods, viz., 8 a.m. to 11 p.m., and 11 p.m. to 8 a.m., which in the following data are referred to respectively as "Day" and "Night." (Fig. 1, Plate XII.)

During the day the temperature was kept at 69° Fahr., and at night at 64° Fahr., by means of the Geissenger electro-thermostat. The thermostat was arranged to operate on a switch which controlled the amount of current supplied to the electric heater aforementioned. In the circuit between the said switch and the heater was placed a wattmeter, which recorded accurately the amount of current consumed by the heater in keeping the room at the desired temperature.

Careful records were taken of the Wattmeter readings as well as the external temperature and that of the incoming air for both day and night periods.

All these data are plotted in the charts on Plate XII., Fig. 1 being the graphical record of the East-bound trip, and Fig. 2 that of the West-bound trip. Summing up the observations taken, the following results were obtained.

Summary of Observations.

Voyage No. 151, *East*, *S.S.* "*St. Paul.*"

	Day.	Night.	Day.	Night.
Average external temperature ...	47·1 deg.	44·8 deg.		
Average temperature of incoming air ventilation	64·3 „	64·4 „		
Actual consumption by Wattmeter			31·9 K.W.	4·8 K.W.
Extra electrical energy consumed by lamps and resistance			9·0 „	3·4
Net consumption			40·9 K.W.	8·2 K.W
Total for trip			49·1 K.W.	

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Voyage No. 152, *West, S.S.* "*St. Paul.*"

	Day.	Night.	Day.	Night.
Average external temperature ...	45·5 deg.	44·7 deg.		
Average temperature of incoming air (ventilation)	64·6 „	64·8 „		
Average temperature of room ...	69 „	64·9 „		
Actual consumption by Wattmeter			50·6 K.W.	7·1 K.W.
Extra electrical energy in room consumed by lamps and resistance			9·5 „	2·5 „
Net consumption			60·1 K.W.	9·6 K.W.
Total for trip			69·7 K.W.	

NOTE.—The mean amount of current used on the two trips was slightly over 40 per cent. of the heater capacity.

With reference to the remarkable difference in power used in the day and night periods, it should be stated that the consumption for the night is so much smaller than that for the day, because the actual time of observation is much shorter, and, also because the amount of heat required to raise the warmth of the room from the night temperature to the day temperature is comparatively large, and, naturally, it appears in the total reading for the day.

The following observations conducted recently on board the S.S. *Oceanic* should be of interest to naval architects, showing, as they do, the average expenditure of electric energy of a typical and purely electrical system of heating, where the passenger has complete control of the heaters.

The halls and public rooms of the vessel are heated by means of steam coils, whereas the staterooms on the two upper decks are heated by electric heaters, consuming on an average 1,200 watts per hour each. These rooms being on the promenade decks are ventilated by means of ducts, one to each room, having an intake close under the deck in the centre of the outside alleyway or promenade, leading from thence into the side of the deck-house, down the side of stateroom behind the casing, and opening to the room through a ported slide, just behind the electric heater, which is placed under the sofa. The doors have solid panels, movable louvres being fitted at the top of the room between the beams and leading into the hallways.

Through the courtesy of Mr. Blake, Superintendent Engineer of the White Star Line in Southampton, an accurate record was kept of the consumption of current used in heating the staterooms of this vessel during one round voyage, Figs. 5 and 6 (Plate XIII.) being the graphic record of the West and East bound trips respectively.

It is worthy of note here that, though this ship was originally fitted with multiple circuit heaters and switches, giving medium, low, and maximum heat, it was found advisable to substitute simple heaters of maximum capacity, as the finer sized wires of a divided circuit heater frequently burnt out and gave trouble. It was also found that the average passenger did not possess sufficient mechanical knowledge to master

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the graduation of the switch, and was disposed to throw the blame for any extremes of temperature on to this mechanism.

Anyone who has had sea experience will appreciate the foregoing observation of the mental apathy of passengers of all classes; for, if a passenger is to be made comfortable, it must be accomplished without effort on his part, and I believe that by far the greater number of complaints, regarding heating and ventilation, are really due to the passenger's neglect or inability to help himself by the means of regulation provided, in any properly installed system.

As a means of determining the actual amount of current necessary to maintain a given temperature in the rooms of the *Oceanic*, a test was made on January 28, 1908, while the vessel was lying in dock in Southampton, upon a representative room on the shelter deck. This room is 9 ft. by 9 ft. by 8 ft. 3 in., with two square ports and one exposed side; the ventilator and heater being as already described. The heater was tested and found to consume 1,045 watts. The external temperatures were 44½° at the beginning and 42° at the end of the test. The room, which had been closed for three days, was found to be at a temperature of 57½°, and, by deduction, the effect of the internal heat (I) was estimated to be 10° (that is, the room would have been at the standard temperature of 70° if the external temperature had been 60°). The heater was run full power till the temperature reached 69½°, when, after several tests, it was found that, if the heater were on for a period of two minutes and off three minutes, the temperature remained constant at 69½°, the radiation expressed in terms of electrical energy was therefore determined to be 418 watts per hour, when the external temperature was 42° and the internal heat, as already stated, 10°, and for this special room may be expressed by the equation W=24 (R—E—I), where W equals watts per hour, R equals temperature at which the room is to be maintained, E is the external temperature, and I the internal heat.

It was also found that 520 watt hours were required to raise the temperature of the room from 64°, the night temperature, to 69°, the day temperature, in addition to the amount radiated.

It will be noted that I have plotted on the chart of the *Oceanic* a curve of theoretical consumption of energy based on the foregoing formula, with an assumed temperature for room of 69° during the day, and 64° at night; and, in view of the large difference between the theoretical and observed consumption of energy, it may be well to consider in what way the conditions at sea may modify the observations of heat dissipation taken in port.

The ventilation of the room, depending as it does on the force and direction of the wind, is, no doubt, greater at sea than in port; but, assuming that the extra amount of air entering the room was 10 cubic feet per minute, the consumption factor would be increased by 4 (R—E), or, approximately, from 24 to 28, or about 16 per cent.

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On the other hand, the internal heat (I) would be naturally larger with the boilers in full operation. On the S.S. *St. Paul* I has the values of 8 and 12 in port and at sea respectively. This correction, if applied to the *Oceanic*, would increase I from 10 to 15, and decrease W by 120, or about 28 per cent. The adjoining rooms and hallways would also be at a higher temperature; therefore, it seems safe to assume that the estimation of electric energy in port is in excess of the sea requirements.

The records of actual consumption were obtained by running the heaters of the staterooms from one machine during the entire voyage, and reading the ammeter every two hours. The external temperatures were also taken every two hours.

The S.S. *St. Paul*, sailing from both ports three days later than the *Oceanic*, encountered different temperatures, and the consumption, as recorded for comparison, is corrected for these differences, the equation for this vessel being W 20 (R—E—I) for radiation, + W 20 (R—A) for ventilation, where I = 12 and A = the temperature of the incoming air. The graphic record for the *St. Paul* is shown in Figs. 3 and 4 (Plate XII.).

SUMMARY OF OBSERVATIONS ON S.S. "OCEANIC."

	Voyage West.		Voyage East.	
	Day.	Night.	Day.	Night.
Observed consumption of electrical energy in watts, S.S. *Oceanic*	872	844	672	644
Calculated consumption, S.S. *Oceanic* ...	555	375	174	49
Observed consumption on S.S. *St. Paul*, automatic control corrected for difference in temperature	536	296	191	0
Apparent waste on each room, S.S. *Oceanic*	257	467	498	597
Waste on voyage of seven days each room in kilowatts	27·0	31·9	52·2	34·5
Total waste night and day in kilowatts ...	58·9		86·7	
Number of rooms occupied	48		50	
Total waste on rooms occupied in kilowatts	2,827		4,340	

In presenting these records to the Institution, I have divided the consumption by the number of rooms occupied on the trip by passengers. It is obvious that at no time were all the heaters in use, owing to the wish of some of the occupants for cold air, and that the reduction to an average of one room is rather unfair to a comparison of a definitely heated room, however valuable to the engineer, as an average daily record of power used.

The records of the *Oceanic* are especially interesting, in that they show the consumption of energy in two extreme conditions. On the West-bound voyage this ship

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passed through exceptionally cold weather, as evidenced by the average temperature, 37·6 and 35·8 day and night, whereas, on the East-bound passage, the average temperatures were 53 and 52·4, temperatures more in keeping with the month of May.

In this connection it is surprising to find such a small difference in the consumption of power between the two voyages, thus proving the statement already made, that the average passenger does not use his heater with reason, or even cater for his own comfort in the smallest degree.

It will be noticed that the corrected consumption of the S.S. *St. Paul* comes into very close agreement with that calculated for the *Oceanic*, and verifies most strongly the basis on which the latter calculations were made.

On the charts of the S.S. *St. Paul* (Plate XII.) the top lines show the temperatures of the room, ventilating air, and external air. The room temperatures are shown for convenience as sharply broken lines, though the change from high to low, and the reverse, necessarily takes some time. The other temperatures are recorded in curves joining the average for the period, and do not show the hourly variations that are actually found in the ventilating air, when steam is turned on.

Combining the known data as to engines, dynamos, and cost of coal with the results of the observations above referred to, we obtain the following results :—

Source of Power : Compound direct connected dynamos of about 90 H.P.

Engine efficiency... ...	... 85 per cent.	... Coal per I.H.P.	... 3·5 lbs.
Dynamo efficiency ...	... 90 per cent.	... „ „ Electr. H.P.	... 4·4 „
Electrical net efficiency ...	... 76½ per cent.	... „ „ Kilo-Watt	... 5·9 „

Cost of coal in bunkers, $4·00 (16s. 5d.) per ton, 2 c. (1d.) per lb.
„ „ „ per K.W.-hour, say, 1·2 c. (2·4 farthings).

Taking the cost for coal at 1·2 c. per K.W., which is certainly a low estimate, and applying it to the estimated waste of electrical energy, in the case of the *Oceanic*, uncontrolled, we get, for the West-bound voyage a loss of $33·92, and East-bound $52, or on the round voyage, say, $86, or £17 13s., in heating forty-nine staterooms. For seven round voyages per year, during which heat is certainly advisable, we get a total of £123 11s. per annum, which might be saved by placing the heaters under automatic control, so that, regardless altogether of the luxury of definite temperatures, the subject becomes a matter of some financial importance.

To apply this system of thermostat control as an adjunct to the thermotank system, it only becomes necessary to fit a specially designed electrical arrangement for operating the steam supply to the thermotanks supplying air to the staterooms, this arrangement being under the control of a Geissenger electro-thermostat, adjusted to regulate the temperature in the air ducts, so as to maintain a comfortable temperature in the inside rooms. To attain this object the temperature of the air

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leaving the thermotank can be kept constant, or the thermostat so arranged as to maintain a constant difference of temperature between the said air and that outside as may be found most effective, the additional heat for the outside rooms being supplied by individual electrical heaters under the control of thermostats placed in each stateroom.

A glance at the graphic record of temperatures on the two trips of the S.S. *St. Paul* at once shows the inability of the engineering staff to properly regulate the temperature of the air as supplied to the vessel, so as to compensate for the changes of temperature outside. In vessels of larger size, where the number of thermotanks is very considerable, the necessity for automatic control becomes at once apparent.

DISCUSSION.

Mr. A. W. Stewart (Member): As Mr. Wallace has mentioned the thermotank system of heating and ventilation in his very interesting paper, perhaps I may be excused for making a few remarks on the subject. I am very much interested in the observations he has made on the electric heaters fitted in the steamers *St. Paul* and *Oceanic*, and would be glad to know what type of heaters were used in each case, whether of the glow-lamp or radiant-heat type, as distinguished from the convection type. The radiant heat type is the one we are most familiar with, and its action depends on the dispersal of its heat in the form of rays, like sunshine. These heat rays have very little warming effect on the air through which they pass, although they can heat up to a considerable degree any conducting object in their path, which, in turn, will heat up the air by convection and conduction. The convection type of radiator, on the other hand, is an air warmer, consisting usually of a metal resistance heated up to a moderate temperature, which transmits its heat to the air like an ordinary steam heater. I quite agree with Mr. Wallace's conclusions as to using electric heaters only as an auxiliary for heating purposes, on account of the high cost of operation. I have some notes beside me of a test made to compare the consumption of steam required to heat a given volume of air in a given time to a given temperature for a thermotank and for an electric heater of the convection type. The thermotank was of a small size, and gave an output of 2,600 cubic feet of air per minute, raised 91 deg. F. above the atmospheric temperature with an expenditure of 240 lbs. weight of steam per hour at a steam pressure of 100 lbs. per square inch. This gives us $\frac{2{,}600 \times 91 \times 60}{240}$ = say, 60,000 cubic feet degrees F. per lb. of steam consumed per hour. The convection type of electric heater gave 100 cubic feet of air raised 20 deg. F. above the atmospheric temperature for an expenditure of 675 watts. Assuming that an ordinary electric generating plant, as used on board ship, will take about 30 lbs. weight of steam per B.H.P., then 675 watts would practically require 30 lbs. weight of steam per hour. This gives us $\frac{100 \times 20 \times 60}{30}$ = 4,000 cubic feet degrees F. per lb. of steam consumed per hour. That is to say, for the same volume of air heated to a given temperature in a given time, the electric heater requires $\frac{60{,}000}{4{,}000}$ = 15 times more steam than the thermotank. In order to get a reasonable amount of heating effect from an electric heater on a reasonable consumption of electric current, you must dispense with practically all ventilation. For this reason I am inclined to favour the radiant type of electric radiator with ventilation, as you get warmed directly from its rays without the intermediary of the air, and thus you may feel warm although the thermometer proclaims that it is cold.

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I am glad that Mr. Wallace has called attention to the large difference of temperature which may exist between the inner and outer rooms of a ship, especially in ships of great beam and boiler power, such as the *Lusitania* and *Mauretania*. The difficulties mentioned by Mr. Wallace in these ships have been completely overcome by the addition of hot and cold air trunks to all the middle rooms, so that a passenger has the means of heating or cooling his room independently of the outer room temperature. The internal heat arising from boiler rooms, &c., mentioned by Mr. Wallace is, to my mind, one of the problems to be faced in ships of large size and boiler power sailing in tropical climates. With ships of great beam it will be impossible to keep the central rooms at a moderate temperature without some artificial means of cooling. I carried out some experiments lately with a thermotank designed for cooling air, and the results were very remarkable. 2,000 cubic feet of air per minute at an inlet temperature of 100 deg. F. were discharged at a temperature of 65 deg. F., the cooling medium used being sea water at a temperature of 56 deg. F., and the quantity used being about 280 gallons per hour. I think that a great deal of useful cooling work could be done by utilising the difference of temperature that exists between the air and sea water, and where the temperature of the sea water preponderates, then brine from the refrigerator could be used.

With regard to the temperature-regulating device described by Mr. Wallace, I am quite certain that if it is reliable in action, it should make a very useful and valuable addition to electric heaters or thermotanks. As to its application to thermotanks, I do not like the idea of automatic steam control, but I should think that it might be more usefully applied to the automatic regulation of the hot and cold air valves. As a matter of fact, it is not advisable to regulate the steam and exhaust valve at all in a thermotank, as, with a steam trap fitted, the valves may be left open all the time, and the air valves will regulate the temperature from extreme heat down to atmospheric temperature.

Mr. William Gray (Member): Mr. Chairman and Gentlemen, I cannot say very much with regard to the electrical device that Mr. Wallace has described in his paper, but I have had a good deal of experience with the system of heating and ventilation that has been adopted in the *Mauretania* and *Lusitania*. Mr. Wallace says, "With regard to the practical working of the system, I believe it has given excellent results as far as the heating and ventilation of the third-class accommodation is concerned, also in the large public rooms and open spaces in the passenger accommodation." He then goes on to describe some of the difficulties encountered in heating the state-rooms, and his proposals for an electric heater rather suggest that the system does not sufficiently lend itself to adjustment in meeting the passengers' requirements in different parts of the ship. Some years ago I had to do with the heating arrangements in some steamers that were designed for a cross-channel service, and they were among the first steamers in which this system of heating and ventilation was adopted in rooms. Formerly it had been fitted in the Atlantic emigrant steamers with large 'tween decks, and it had proved successful. In channel steamers, as is well known, the boilers come close up to the main deck, and the requirements for heating and ventilating cabins in that part of the ship are quite different from those in other parts. This is a somewhat similar case to that to which Mr. Wallace refers, but it has been found in practice that there is no difficulty in regulating the hot air to meet the different requirements, and the whole system has proved an unqualified success.

Mr. Wallace, in another part of his paper, speaks of a lower night temperature as being very desirable from a health point of view. In another case in which I was interested, the training ship *Exmouth*, belonging to the Metropolitan Asylums Board, the system of thermotanks was fitted throughout. There, again, it was a departure from precedent, and the Board's advisers, who had been accustomed to heating hospitals by hot-water pipes, steam, and all sorts of different methods, were not favourably disposed to the introduction of this thermotank system, but it has now been in use in the *Exmouth* for over two years. and it has been pronounced a great success. In a report I have recently received on the subject, it was stated that the health and comfort of the boys had been greatly improved by the introduction of this system as compared with the hot-water system in the earlier vessels. It was specially remarked that it lent itself to careful adjustment in order to meet the varying requirements in different parts of the

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ship at all hours. On these grounds, therefore, I am disposed to think that Mr. Wallace is exaggerating the difficulties that may be encountered, and that, however valuable the electrical apparatus may be, the thermotank system in itself is capable of providing all the necessary adjustment of temperature.

The CHAIRMAN (Mr. S. J. P. Thearle, Member of Council): If there is no one who wishes to make any further remarks, it remains for us to convey—which I think we shall be delighted in doing—to Mr. Wallace the sincere and hearty thanks of this Institution to him for writing this interesting and useful paper.

WRITTEN REPLY TO THE DISCUSSION.

Mr. W. CARLILE WALLACE (Member): I am much obliged to Mr. Stewart and Mr. Gray for the interest they have shown in my paper, which was written with the object of calling forth the views of members on what I consider a most important side issue, so to speak, to naval architecture, and a subject in which we are very far from reaching finality. In the first place, I want to disclaim any attempt on my part to advocate electric heating from the standpoint of economy, as this would be manifestly absurd. In steam heating we only have the boiler losses and the losses of transmission, which, combined, may be, say, 30 or 40 per cent., provided the steam is condensed in the heater. In electric heating we first have to produce our power at a loss of, say, 90 per cent., and then convert this power into heat at a further loss of probably 25 per cent. So that if we get from 6 to 8 per cent. of the heat value of the coal burned under the boilers, we are doing well. From this it will be seen that Mr. Stewart's estimate closely agrees with mine, if the losses in the air ducts are considered. On the other hand, I do claim that in the better-class rooms individual heating is most satisfactory, and if this is granted, then electrical heating is in every way more convenient and better than steam. It is the excessive cost of electrical heating which makes it so important to place it under automatic control, when by so doing it is possible to save 50 per cent. of the electrical energy. The electrical heaters in the *St. Paul* and *Oceanic* are of the convection heater type. This form of heater really raises the temperature of the air, whereas the glow-lamp or radiant heater simply appeals to the imagination through the sense of sight, it being the Englishman's idea of an open fire; but you have to get close to it to feel the effect. Both speakers altogether overlook the variations in the ideas of passengers as to what is a comfortable temperature, and this is one of the most serious difficulties to be met where a general system of heating is adopted, especially in vessels engaged in the North Atlantic trade. I am entirely favourable to the thermotank system of heating, and believe that by leading warm and cold air ducts to each room and intelligently manipulating the valves attached to the same, any degree of heat may be obtained. The objections to this system are the lack of space for double ducts of ample area and the lack of intelligence possessed by the average passenger where anything mechanical is concerned. Granted that it is possible to fit ducts of sufficient area, then, by placing the valves under automatic control, the object sought will be obtained —namely, a definite constant temperature independent of outside weather conditions, which in the North Atlantic change very rapidly.

I am quite at one with Mr. Stewart as to the problem to be faced in keeping the inside rooms in vessels of large power and great beam at a moderate temperature in warm weather, and am much interested in the account of his experiments in cooling air by passing it through a specially designed thermotank cooled by sea-water, which, if practicable, would be a great boon in hot weather. I regret that Mr. Stewart did not give some further information on the subject, such as the relative humidity of the air when entering and leaving the cooling tanks, and whether there was a large deposit of water in the ducts through which the cooled air was led. Unfortunately, on the North Atlantic passengers suffer most from heat in the Gulf Stream in summer, when both the water and air are at a temperature of 80°, the humidity of the latter being almost saturation; under these conditions, any attempt to cool the air by use of brine from the refrigerator would cause such a deposit of moisture on both the inside and outside of the air ducts that they would be very rapidly destroyed by corrosion, the air being delivered to the rooms at the point of saturation.

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THE INFLUENCE OF AIR ON VACUUM IN SURFACE CONDENSERS.

By D. B. MORISON, Esq., Member of Council.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 9, 1908; The Hon. C. A. PARSONS, C.B., F.R.S., D.Sc., M.A., Member of Council, in the Chair.]

A PAPER was read by Professor R. L. Weighton before this Institution* in April, 1906, the essential object of which was to demonstrate the effect of design on the efficiency of surface condensers, with particular reference to a series of tests carried out at Armstrong College, Newcastle. The present paper deals with the effect of air on vacuum in surface condensers.

SATURATED AIR.—If a condenser contained steam only and no air, the vacuum would be governed by the mean temperature of the steam throughout the condenser, and an air pump would not be required. In practice, air exists in all steam condensers; its presence has the effect of retarding the condensation of the steam on the tube surface, and it has to be removed in a state of saturation with water vapour by an air pump.

When a condenser is at work and a stable condition has been established, the weight of air entering and leaving it in a given time is the same; but the ratio of air to steam in different parts is very variable. At the steam inlet the proportion of air to steam is, in a reasonably air-tight system, so small that its effect is negligible. As the fluid passes through the condenser and the steam condenses, the ratio of air to steam increases rapidly, until at the air-pump suction the air forms a very considerable proportion of the mixture. It is customary to refer to the fluid entering the condenser as "steam," and to the fluid leaving it as "air," but the two are of precisely the same nature, and differ only in the proportion of the constituents. It is true that what is termed air may not have exactly the composition of atmospheric air, but this is a point of little consequence in the present investigation.

TABLE I.—There is, as is well known, a definite temperature corresponding to any pressure of saturated steam. This is given by Table I. (page 162), a column representing vacuum expressed as inches of mercury being added to the usual columns expressing absolute pressure and temperature. But there is also a definite—and lower—temperature corresponding to every proportion of air to steam in saturated air at every pressure.

TABLE II.—This table gives the vacuum and corresponding temperature of airless steam and also of three proportions of air to steam, which are merely chosen to exemplify

* Trans. I.N.A., Vol. XLVIII. p. 123.

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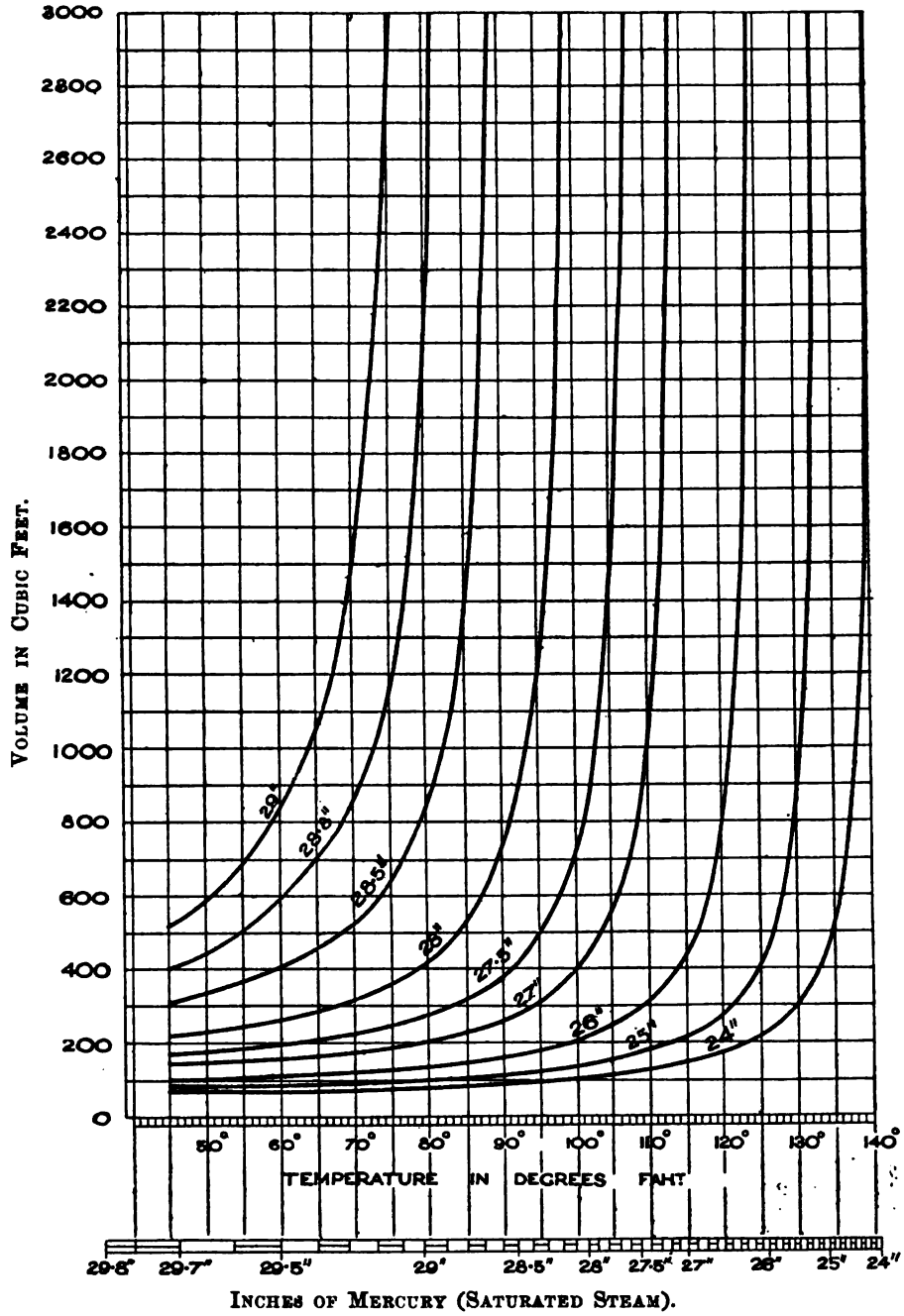
DIAGRAM I.

VOLUME OF 1 LB. OF AIR WHICH IS SATURATED WITH WATER VAPOUR AT VARIOUS TEMPERATURES AND VACUA.



the principle. It will be seen from these tables and from Diagram II. (page 153) that for a given vacuum the fall in temperature of the gaseous mixture which passes through a condenser and on to the air pump changes with the ratio of air to steam in

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a perfectly definite manner; and this property, which has not hitherto received the recognition it deserves, has an important bearing on all investigations on this subject.

Diagram I.—I have found this diagram of great service in dealing with condenser and air-pump problems, and a brief explanation regarding it, and also Table III., which expresses the same results in tabular form, may be of interest. The first column in the Table is one of temperatures, and the second column gives the pressure of water vapour corresponding to these temperatures. This relation between the pressure and the temperature of steam has already been given in Table I., and is, of course, well known.

It is important to note that, if water vapour is mixed with another gas or gases, the total pressure is the sum of the pressures of the several constituents, the pressure of each of which may be termed the partial pressure. The partial pressure of the water vapour is dependent only on the temperature; it is unaffected by the presence of the other gas or gases. The third and fourth columns refer to a vacuum of 24 in. of mercury when the barometer is at 30 in., the corresponding absolute pressure being 2·95 lbs. per square inch. If air saturated with water vapour is at a temperature of, say, 50° Fahr., and if its total pressure is 2·95 lbs., then, as the partial pressure of water vapour at this temperature is 0·17 lb., the partial pressure of the air must be the difference between these figures, *i.e.*, $2{\cdot}95 - 0{\cdot}17 = 2{\cdot}78$ lbs. per square inch, as given at the top of the third column of the table. The volume of air is, as is well known, dependent on its temperature and pressure, and can be readily calculated if these data are given. The fourth column of the table gives the volume of 1 lb. of saturated air. This has been calculated from the partial pressure of the air; the volume of the water vapour which occupies the space coincidentally with the air does not affect the result. The succeeding columns give the partial pressures and volumes of air which is saturated with water vapour at various other vacua, and Diagram I. shows the volumes graphically. A scale of vacua and temperatures for saturated steam is, for convenience, given at the bottom of the figure.

Air Ratio.—It was mentioned above that, as the steam passed through the condenser, it became increasingly rich in air. As this increasing air richness reduces the rate of transfer of heat from the steam to the water, it is desirable to show it graphically, and Diagram II. has been prepared from Diagram I. for this purpose. The scales of volume and temperature at the left hand side and bottom respectively of the diagram are the same as in Diagram I., as are also the lines representing the different vacua; but additional curves have been drawn representing the approximate ratio of air to steam. In Diagram II. it will be seen that, as the vacuum lines are followed to the right and upwards, the ratio of air to vapour falls, and, if the lines were continued to an infinite height, the ratio of air to vapour would be zero, *i.e.*, the fluid would be saturated steam. The (vertical) temperature lines representing the boiling points of water at various vacua are therefore asymptotic to the vacuum curves. For example, the temperature ordinate for 85° is asymptotic to the vacuum

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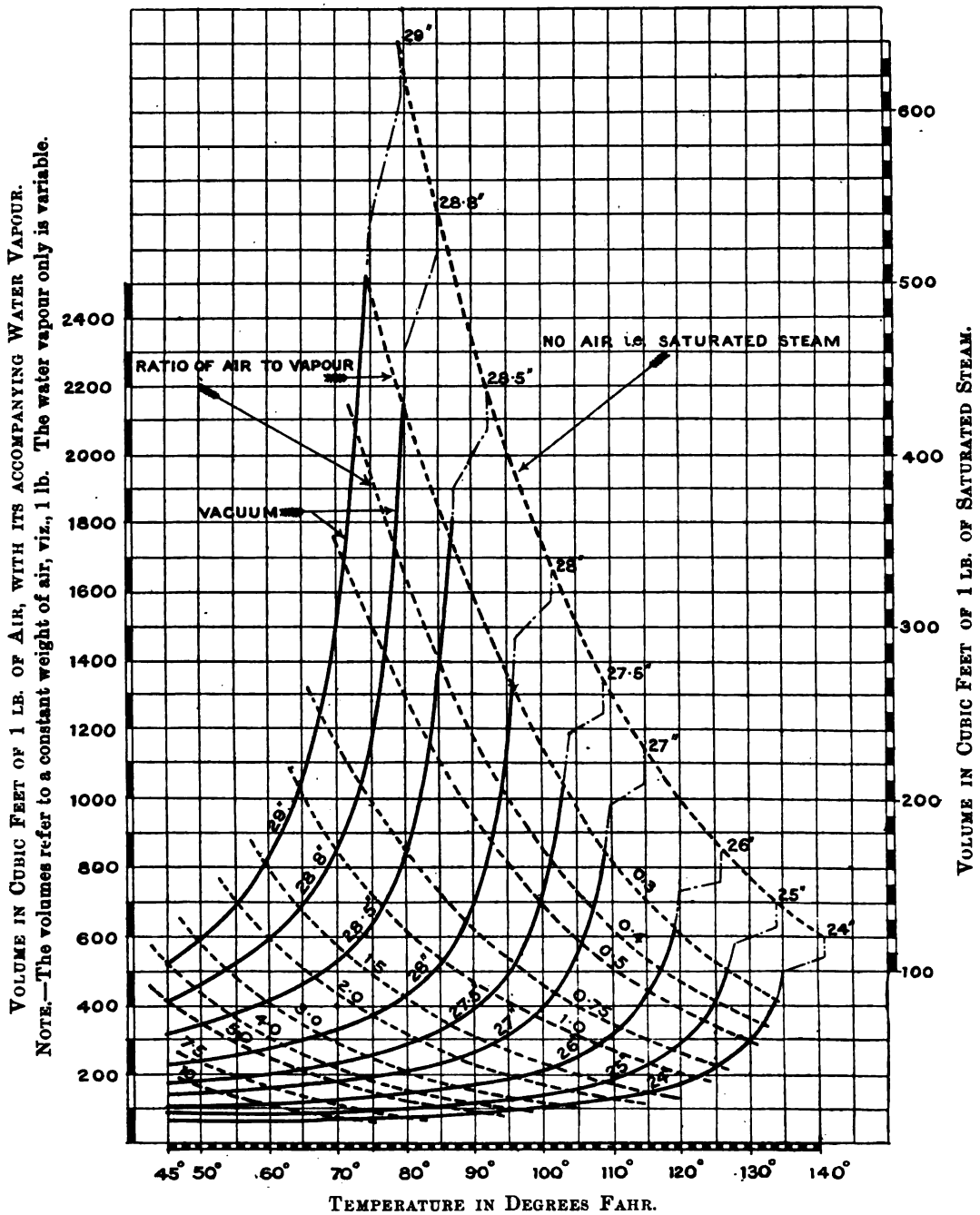
DIAGRAM II.

AIR SATURATED WITH WATER VAPOUR.

CURVES SHOWING RELATION BETWEEN VACUUM, TEMPERATURE, VOLUME, AND RATIO OF AIR TO VAPOUR.



line for 28·8 in., as the boiling point of water at 28·8 in. vacuum is 85° F. The vacuum curves have been broken towards their upper ends to bring them into coincidence with their asymptotes, the top ratio curve, situated above the broken parts,

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representing saturated steam to a scale of volumes given at the right-hand side of the diagram.

For any steam condenser the varying condition of the fluid when passing through it from steam inlet to air exit is obtained by following down the vacuum curve representing the vacuum in the condenser; starting on the diagram from practically saturated steam, the curve is followed until a point is reached determined by the air pump, the relative capacity of the latter per lb. of air being indicated by the vertical scale of volumes.

Capacity of Air Pump.—The necessary capacity of the air pump, therefore, depends on the point at which the air and steam are withdrawn, and a withdrawal far down on the vacuum curve would involve a relatively small air pump. But, as the transmission of heat through the metal of the condenser tubes from the steam to the water depends on the difference of temperature between these fluids, it is necessary, in order to obviate the necessity of unreasonably large condensers, to withdraw the air and vapour before the temperature of the mixture falls unduly. Hence it can be said that, for a given condensing surface and given steam and circulating water conditions, there is a limit in air ratio below which the mixture should not be carried; and this limit determines the minimum capacity of air pump necessary per lb. of steam in order to condense the required amount of steam per square foot of cooling surface at the required vacuum.

Effect of Air.—Even with an air-pump capacity in proportion to the amount of air gaining access to a condenser, the efficiency of the condensing surface must be adversely affected by increased quantities of air passing through, because the opportunities for contact of the steam with the tube surface are thereby reduced. In connection with the variation in the condensation rate with varying air richness, the most noteworthy experiments are those of Mr. Jas. A. Smith, made in Australia, the results of which are given in a paper read by him before the Victorian Institute of Engineers in 1905; but, as in these experiments the steam had little velocity and there was no air pump, the conditions were very favourable to the accumulation of a film or zone of air around each condensing tube. In the absence of other experiments, it is impossible to give figures for the effect of air richness on the condensation rate in condensers working under vacua usual in steam engine practice, but the fact is established that increased air leakage is always associated with decreased heat transmission through the tube surface.

Ineffective Condensing Surface.—If the air pump capacity is not increased in proportion to an increase in air leakage, the lower rows of tubes automatically become ineffective for condensing purposes, and this so reduces the effective surface that a decrease in vacuum is required, and ensues in order to re-establish the necessary equilibrium.

Consider, for example, a mixture of steam and air at a vacuum at 28·5 in. at the bottom or exit end of a condenser through which the circulating water makes its first

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pass. If the relative proportion of air and vapour by weight are as 0·3 to 1, a reference to Diagram II. will show that the temperature of the mixture is about 87°; but, if the air gaining access to the condenser is increased without a corresponding increase in the pump capacity, so that at the bottom of the condenser there are equal weights of air and vapour, the temperature is only 78°, and, if the weight of air is twice that of the vapour, the temperature is only 68°. With circulating water entering the condenser at, say, 60°, and having a mean temperature in the first pass through the lower rows of tubes of, say, 63°, 62°, and 61° respectively in the three cases, the difference in temperature between the condensing water on the inside and the air and steam on the outside of these condenser tubes is 24°, 16°, and 7° respectively, and, as with unflooded tubes the transmission of heat is very nearly proportional to this difference of temperature, the great effect of the quantity of air on the capacity of the condenser will be apparent.

Flooded Tubes.—The above statement was, it will be noted, qualified by the expression "with unflooded tubes," because, with tubes heavily flooded by steam water showering from the top, as is the case in a condenser of ordinary design, the air and steam cannot get access to the tubes; but make contact with this steam water which cannot be at a less temperature than the water within the lower tubes and will, in ordinary cases, as it comes from the upper and hotter portion of the condenser, be at a considerably higher temperature. If the steam water flooding the lower tubes were at a temperature of 77°, then the difference between this and the saturated air would be 10° in the first case considered, and only 1° in the second, which is, therefore, barely practicable, the third case being, of course, impossible; so that in a condenser of ordinary proportions and design in which the condensed steam water showers on to the lowermost tubes, the ratio of air to steam by weight at the air pump suction must be less than unity in order that 28½ in. vacuum may be obtained with circulating water at 60°.

Air Effect on Circulating Water.—A fall in vacuum when caused by air leakage does not materially affect the temperature of the circulating discharge water, because the reduction in heat transmission due to the lower temperature in the condenser base is largely compensated for by the greater transmission resulting from the higher temperature at the top; moreover, any difference in the discharge temperature of the condensing water is readily calculable from the increased temperature of the condensed steam water, and is an illustration of how great a change in condenser efficiency when due to the presence of air may be accompanied by a trifling change in temperature of the circulating discharge water.

Air Leakage.—The weight of air entering the condenser with the steam depends on (*a*) the aggregate air leakage into the system at all parts subjected to pressure below atmospheric, and (*b*) the proportion of air contained by the feed water when entering the boiler.

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In marine practice minimum air leakage is associated with a steam turbine having tight glands, minimum number of joints all easy of access, and water-sealed connections, and in which no engine other than the turbine exhausts into the condenser; maximum air leakage is usually to be found when a reciprocating engine and all its auxiliaries exhaust into one common condenser; and it is no exaggeration to say that the loss in power by the main engine due to air leakage from these auxiliaries often far exceeds their aggregate power, particularly in tropical waters, when the effective size of the condenser is so easily lessened, due to the near approach in temperature between the water within the tubes and the steam and air without the tubes in the lower part of the condenser.

Air in Feed Water.—The weight of air which can enter a boiler with the feed water depends upon the amount of air in solution in the feed water, together with the amount of air added by the feed pump. In marine practice the greatest amount of air results from the use of ordinary marine ram pumps, driven from the air pump crosshead by the main engines, and air is present in minimum amount when the feed pump is independently driven and float-controlled. The weight of air and other non-condensable gas in real or apparent solution in feed water has been the subject of exhaustive investigation by the writer, and the results as affecting marine practice are, broadly, as follows:—

Fresh water carried in tanks for use as auxiliary feed contains at atmospheric pressure from fully 2 to 3½ volumes of air per 100 volumes of water. If this water is introduced into the condenser, about 70 per cent. to 90 per cent. of its air is immediately given off. Water of condensation contains practically no air when withdrawn by a water pump from the upper sections of a vertical type "Contraflo" condenser; but when allowed to shower freely to the bottom of a condenser of ordinary design it absorbs air, the amount depending on the air ratio in the condenser base, and it is further charged to a slight extent in its passage through an air pump. When the entire feed water passes through an air pump, the air in the pump discharge amounts to from 1½ to 2 per cent., depending on the conditions; but, if this water is passed without disturbance into a feed tank and pumped therefrom by a float-control feed pump, it does not take up further air to any appreciable extent. Condensed water discharged by an air pump and re-passed into the vacuum does not become supercharged by repeated circulation through the system, but settles down to a permanent charge of about 2 per cent. which is slightly less than the average quantity contained in ordinary fresh water.

In order to show how this air affects the air pump capacity, Tables IV. and V. (page 164) have been prepared. The first column in each of these tables represents the volume of free air per 100 volumes of water; the remainder of each table is self-explanatory, the last column representing the volumes to be dealt with by the air pump.

Air Pump Effect.—Broadly, it may be said that in no type of condenser can the condensing capacity due to its dimensions be maintained, unless the air pump is

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sufficient to render all the tube surface available for condensing steam; the air pump must, in fact, dominate the condenser under all conditions of normal air leakage as the least insufficiency so re-acts on the condenser as to ultimately put out of action a proportion of the entire condensing surface, with the result of a fall in vacuum. This suppression of effective condensing surface is usually at a maximum in tropical waters, and results to some extent from the prevailing practice of adopting the average temperature of home waters as a basis for determining the general proportions of a condensing plant. To obtain relatively high vacua in low temperature water is no evidence whatever that a plant is correctly designed; and, in order to obtain maximum average efficiency at all temperatures within the usual range, it is necessary to proportion the tube surface, circulating water, and air pump for the highest temperature conditions in which a ship may be required to steam, and this is particularly the case in turbine engined war vessels, because failure to obtain the best vacuum for a given weight, space occupied, and pumping power is equivalent to a definite sacrifice of either power and speed or radius of action.

Air Measurement.—A condensing plant may be said to be in an ideal condition when the only air in the system is that discharged into the boiler in solution with the feed water, but in general practice there must of necessity be considerable air leakage associated with every plant, the amount being largely determined in any given case by the condition in which the plant can be maintained with average attention by those in charge. This may be termed the normal leakage of the plant, and to reduce it to a basis of cubic feet of air at atmospheric pressure per unit of time has long been the desire of all those interested in the problem of steam condensation under vacuum. Apparatus to achieve this object has lately been devised, and already it has proved of great assistance in the investigation of this subject, and will undoubtedly be of great utility in ordinary practice. By its use a minimum normal air leakage may be obtained and maintained, as any additional leakage, however small, is immediately indicated; so that air pumps can in future be proportioned for an air leakage which by experience has been proved to be the normal for any given plant, instead of by the haphazard methods now frequently adopted, which so often lead to disastrous effects on condenser efficiency.

Take, for example, the case of a fleet of destroyers of practically identical machinery design: it is at present quite impossible to ascertain or compare the air leakages in the various units, or to determine what is the normal air leakage and whether the air pump is sufficient. In future, however, a fair normal can be established, and thereafter adopted as a basis for all boats of a particular class. The writer's experience of the apparatus used in connection with a Parsons turbine at Hartlepool has been that what was formerly considered to be a fair average air leakage has been reduced by fully 50 per cent., and the attendants have no difficulty in maintaining the air tightness, so that the ultimate effect of this invention on turbine economy is obvious; but, whatever the type of engine and whatever the desired vacuum, ability to readily and visibly

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ascertain the amount of air leakage at any moment must always be associated with definitely improved results.

Another valuable feature in the apparatus is that, by mechanically admitting air to a condenser through a graduated valve or otherwise, and observing the discharge necessary to produce a given fall in vacuum, it can at once be seen whether the air pump has a margin in reserve, and its amount ascertained.

Air-Pump Capacity.—The writer regrets that it is impossible within the limits of this paper to deal in detail with apparatus for the withdrawal of air, except in so far as air-pump efficiency is affected by condensers. The effective capacity of a given air pump being determined by the amount of pressure reduction created within its barrel relatively to the pressure in the condenser, it follows that the temperature of the water discharged by the pump determines the limit of such reduction, because on the pressure-reducing stroke the contained water evaporates immediately the boiling point is reached. So instantaneous is the evaporation and condensation on the pressure-reducing and pressure-increasing strokes of an air pump that, in some experiments made by Mr. Fred Edwards with an Edwards pump having a glass barrel, the glass became alternately dull and clear with a rapidity which the eye could scarcely follow.

Reference to Diagram II. (page 153) will show how inappreciable is the effect of increase in volumetric capacity of air pump when the temperature at its suction approaches the temperature corresponding to the vacuum, and how great is the effect when the temperature difference increases. An ordinary condenser and air pump form an unfortunate combination in this respect, as any increase in load is followed by higher temperature feed water, and consequent reduction in air-pump effect, just at the time when greater capacity is desirable. In this connection it may be observed that such a design of condenser as will ensure a maximum and uniform air richness at the air pump suction allows of the employment of an air pump of minimum capacity, so that any design in which the line of flow from the steam inlet to the air outlet represents a zone of maximum temperature cannot be conducive to air-pump efficiency. Whenever conditions permit, it is obviously technically correct practice to withdraw the condensed steam water and the air and vapour by separate pumps, thereby obtaining thermal and air-pump effects quite unattainable when with ordinary air leakages an air pump alone is used, and, as the importance of this question is realised, future development will undoubtedly be along these lines. An air pump should be of such proportions as will render the lowermost tubes in a condenser effective for condensing steam under normal conditions of air leakage, and with the maximum temperature of circulating water, and beyond this there should, whenever possible, be a margin for contingencies.

Air-Pump Efficiency.—Diagram III. (page 159) is based on actual results obtained from a series of tests made on air pumps, but is somewhat sharply defined for purposes of explanation. The two pumps represented by A, B, were of equal size and standard of workmanship, but different types. They were tested with a condenser connected direct to a boiler, and air to a predetermined amount was admitted by mechanical

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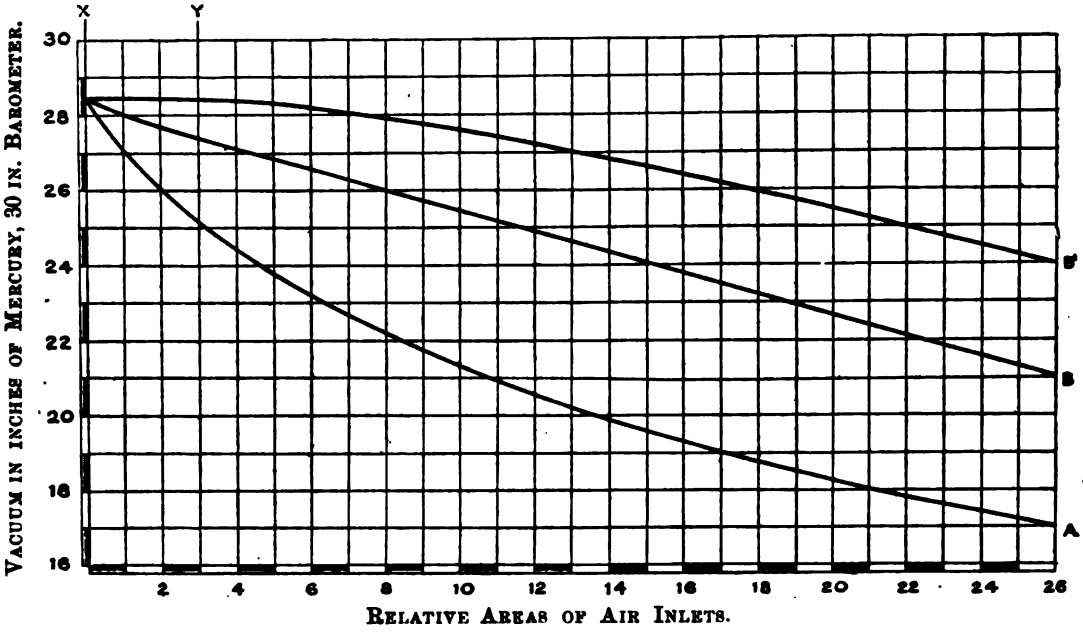
means. The vertical scale on the diagram represents vacuum and the horizontal increasing quantities of air admitted. Steam and condensing water were constant throughout the tests, nothing being altered except the areas for air admission. With no air admission both pumps produced the vacuum corresponding to the temperature conditions in the condenser, viz., 28½ in. On the opening of the smallest air port the vacuum with pump A fell quickly, and with the largest port open the fall amounted to 11½ in. or to 17 in. Under the same conditions the vacuum with pump B fell much more slowly with the small port open, and with the largest port open the fall was 7½ in. or to 21 in. Neither pump was capable of maintaining the full vacuum when even a very small quantity of air was admitted, but the difference in air-withdrawing efficiency between the two types of pump is very definite.

DIAGRAM III.



LOWERING FEED WATER TEMPERATURE.—In land installations it is common practice to allow all the feed water to accumulate in the condenser bottom, and so form an open well or pond through which the cooling tubes pass. This is with the object of increasing the effective capacity of the air pump by lowering the temperature of the water passing through it; but, when conditions are such that the air pump is sufficient, the assistance of a cooler is not required; so, with the object of giving flexibility to the air-pump capacity, and thereby conserving heat in the feed water, the system shown in Fig. 1 (Plate XIV.) has been devised.

CONTROLLABLE COOLING SYSTEM.—The condenser illustrated is one of the "Contraflo" vertical type as applied to the triple-expansion engines of a cargo boat. The cooler is situated in the base of the condenser, from which it is separated by a diaphragm, and is normally kept quite filled with water. When there is a

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sufficiency of air-pump capacity, as will generally be the case in cold waters, all the steam water passes directly to the air pump in the ordinary manner and at a high thermal value, the only effect of the cooler being to advantageously lower the temperature of the condenser bottom; but when in tropical waters, or in the event of abnormal air leakage, the whole or any desired proportion of the feed water may be passed through the cooler, the amount depending on requirements, so that of the total range of the cooler, only such a proportion of it is used as will prevent the air pump from prejudicially re-acting on the efficiency of the condenser. Referring again to Diagram III., the application of the cooler system to pump B has a marked effect, and the results are shown by B′. Not only are the vacua very appreciably higher throughout, but the pump has now an actual margin in capacity as represented by x, y, and is able to maintain the full vacuum under all air leakages within that margin. The horizontal x, y may, therefore, be termed air-pump reserve due to the cooler, and this reserve need only be drawn on as and when required. The general effects produced are of such technical interest that exhaustive tests are now being made and the results will be dealt with on some future occasion.

Figs. 2 and 3 (Plate XV.) illustrate the system applied to the condensers of turbine-engined destroyers, and, as in this case, the highest vacuum is always desirable, the whole of the steam water from the upper section passes through the cooler.

Fig. 4 illustrates a battleship condenser, so arranged that the steam water is withdrawn by a water pump at the highest possible thermal value, and in a state of minimum aëration. The water necessary for the air pump passes through the cooler and the pump system in continuous circulation, any surplus being bye-passed into the steam space of the condenser. This arrangement, besides resulting in very high thermal efficiency, gives considerable flexibility to the main air pumps, as the constant and uniform water supply naturally promotes a great regularity in working, and admits of a high range of speed when occasion requires. Alternatively, this condenser may be arranged with a controllable cooler, as in Fig. 5, in which case a separate water pump is not required. Fig. 6 illustrates a condenser without a combined cooler, and in which the flow of steam promotes maximum and uniform air richness at the air-pump suction. It is impossible to standardise condensing plants, however, as every proposition must be based on the particular conditions prevailing.

All these condensers embody the distinguishing features of the design, viz., maintenance of uniform flow at right angles, both to the axes of the tubes and to the falling streams of condensed steam water, the depth of fall being determined by the area necessary for the flow of steam between the rows of tubes, and small relatively to the length of flow from the steam inlet to the air outlet. Fig. 7 shows in section a vertical "Contraflo" design having four water passes, and Fig. 8 is the corresponding horizontal arrangement. The latter has been found very convenient for experimental purposes, as, if the base is divided up into cross channels each separately drained,

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facilities are afforded for the measurement of the rate of condensation on any row or group of tubes.

The writer has found it very difficult to crowd into a short paper the experiences of a very prolonged research in connection with this subject; and, with a view to avoiding repetition, it has been assumed that this contribution will be read in conjunction with the paper on condensers submitted to this Institution in 1906.

In conclusion, the writer desires to record his thanks to Professor Weighton and the research staff at Armstrong College, Newcastle, and to Mr. R. M. Neilson and to other members of Messrs. Richardsons, Westgarth's technical staff at Hartlepool, for their assistance in the preparation of this paper.

TABLE II.

TEMPERATURES AND PRESSURES OF SATURATED AIR.

Vacuum. Inches of mercury with bar. at 30 in.	Proportions of Air and Steam by Weight.				
	No Air, *i.e.* Sat. Steam.	Air 0·25. Steam 1·0.	Air 0·5. Steam 1·0.	Air 0·75. Steam 1·0.	Air 1·0. Steam 1·0.
Inches.	Degrees F.	Degrees F.	Degrees F.	Degrees F.	Degrees F.
29	79·5	75	71	67·5	64·5
28	101·5	96·5	92·4	88·8	85·3
27	115	110	105·6	101·7	98·6
26	126	120·2	115·5	111·5	108·3
25	134	128·4	123·5	119·2	116·2
24	141	135·2	130·3	125·8	122·3

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TABLE I.

Temperatures and Pressures of Saturated Steam.

Absol. pressure. Lbs. per square inch.	Temperature. Degrees Fahr.	Vacuum. Inches of mercury when bar. is at 30 in.	Absol. pressure. Lbs. per square inch.	Temperature. Degrees Fahr.	Vacuum. Inches of mercury when bar. is at 30 in.
0·0982	36	29·8	1·277	110·5	27·4
0·1474	46	29·7	1·326	111·5	27·3
0·1965	53·5	29·6	1·375	113	27·2
0·2456	60	29·5	1·424	114	27·1
0·2947	65	29·4	1·474	115	27·0
0·3438	69	29·3	1·523	116·5	26·9
0·3929	73	29·2	1·572	117·5	26·8
0·4421	76	29·1	1·621	118·5	26·7
0·4912	79·5	29·0	1·670	119·5	26·6
0·5403	82·5	28·9	1·719	121	26·5
0·5894	85	28·8	1·768	122	26·4
0·6385	87·5	28·7	1·817	123	26·3
0·6876	90	28·6	1·866	124	26·2
0·7368	92	28·5	1·916	125	26·1
0·7859	94	28·4	1·965	126	26·0
0·8350	96	28·3	2·063	128	25·8
0·8841	98	28·2	2·161	129	25·6
0·9332	99·5	28·1	2·259	131	25·4
0·9823	101·5	28·0	2·358	133	25·2
1·031	103	27·9	2·456	134	25·0
1·081	104·5	27·8	2·554	135·5	24·8
1·130	106	27·7	2·652	137	24·6
1·179	107·5	27·6	2·751	138	24·4
1·228	109	27·5	2·849	139·5	24·2

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TABLE III.

VOLUMES OF AIR SATURATED WITH WATER VAPOUR AT VARIOUS TEMPERATURES AND VACUA.

Temp. in Degs. Fahr.	Pressure of Water Vapour at this Temp.	Vacuum 24 in. corresponding to 2·947 lbs. per sq. in.		Vacuum 26 in. corresponding to 1·965 lbs. per sq. in.		Vacuum 27 in. corresponding to 1·474 lbs. per sq. in.		Vacuum 27·5 in. corresponding to 1·228 lbs. per sq. in.		Vacuum 28 in. corresponding to 0·9823 lb. per sq. in.		Vacuum 28·5 in. corresponding to 0·7368 lb. per sq. in.		Vacuum 28·8 in. corresponding to 0·5894 lb. per sq. in.		Vacuum 29 in. corresponding to 0·4912 lb. per sq. in.	
		Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.	Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.	Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.	Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.	Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.	Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.	Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.	Partial Pressure of Air.	Volume of 1 lb. of Air with accompanying Vapour.
	Lbs. per sq. in.	Lbs. per sq. in.	Cubic ft.	Lbs. per sq. in	Cubic ft.	Lbs. per sq. in.	Cubic ft.	Lbs. per sq. in.	Cubic ft	Lbs. per sq. in.	Cubic ft.	Lbs. per sq. in.	Cubic ft.	Lbs. per sq. in.	Cubic ft.	Lbs. per sq. in.	Cubic ft.
50°	0·17	2·78	68	1·79	105	1·30	147	1·06	176	0·81	233	0·57	336	0·42	450	0·32	592
60°	0·25	2·70	71	1·71	113	1·22	158	0·98	197	0·73	263	0·49	393	0·34	566	0·24	800
70°	0·36	2·59	75	1·60	124	1·11	178	0·87	226 266	0·62	315	0·38	520	0·23	852	0·13	1536
80°	0·50	2·45	81	1·46	137	0·97	204	0·73	273	0·48	420	0·24	832	0·09	over 2000	—	—
90°	0·69	2·26	90	1·27	163	0·78	260	0·54	379	0·29	700	0·05	about 4000	—	—	—	—
100°	0·94	2·01	103	1·02	203	0·53	390	0·28	727	0·042	nearly 5000	—	—	—	—	—	—
110°	1·26	1·69	125	0·70	304	0·21	over 1000	—	—	—	—	—	—	—	—	—	—
120°	1·68	1·27	170	0·28	770	—	—	—	—	—	—	—	—	—	—	—	—

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TABLE IV.

Free air at 60° F. and atmospheric pressure per 100 cubic feet of water.	Volume of same air at 60° F. and 27 in. vacuum.	Volume of same air at 105° F.* and 27 in. vacuum.	Same air but saturated with water vapour at 105° F. and 27 in. vacuum.
Cubic feet.	Cubic feet.	Cubic feet.	Cubic feet.
0·5	5·0	5·4	22·4
1·0	10·0	10·9	44·9
1·5	15·0	16·3	67·3
2·0	20·0	21·8	89·8
2·5	25·0	27·2	114·1
3·0	30·0	32·6	134·7
3·5	35·0	38·1	157·0
4·0	40·0	43·5	179·5

TABLE V.

Free air at 60° F. and atmospheric pressure per 100 cubic feet of water.	Volume of same air at 60° F. and 29 in. vacuum.	Volume of same air at 70° F.* and 29 in. vacuum.	Same air but saturated with water vapour at 70° F. and 29 in. vacuum.
Cubic feet.	Cubic feet.	Cubic feet.	Cubic feet.
0·5	15·0	15·3	62
1·0	30·0	30·5	124·3
1·5	45·0	45·8	186
2·0	60·0	61·1	249
2·5	75·0	76·4	311
3·0	90·0	91·5	373
3·5	105·0	107·0	435
4·0	120·0	122·2	497

* *I.e.* about 10° F. below the temperature of saturated steam at the same pressure.

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DISCUSSION.

Professor R. L. WEIGHTON, M.A. (Member): Mr. Chairman and Gentlemen, the subject-matter of the paper before us forms a very important part of the general problem as to what makes for condenser efficiency. In general terms it may be said that that condenser is the most efficient which, under given conditions, maintains the lowest mean temperature on the steam side of the condensing surface. This will clearly be the condenser which, in the given conditions—including, of course, the air-pump—will maintain the highest vacuum. But this again involves the transference, in unit time per unit surface, of the maximum number of thermal units from the steam to the condensing water, with minimum temperature difference between the steam and water sides of the surface. Hence, maximum condenser efficiency is in the last resort determined by the efficiency of the *surface*. And, indeed, it is evident that *surface efficiency* is at the root of every possible efficiency in a surface condenser. Now, what are the conditions under which surface efficiency will be at a maximum? I venture to think there will be general agreement as to the answer to this question. Maximum efficiency will occur when the surface on the one side is directly and freely accessible—without any intermediary—to the medium which is parting with its heat, and on the other side to the medium which is absorbing that heat. When both the steam to be condensed and the condensing water have the freest and most direct and unimpeded contact with bare metallic surfaces, the rate of heat transmission and absorption, under given conditions otherwise, will be at a maximum, the mean temperature inside the condenser will be at the lowest and the vacuum at the highest.

Several influences may conduce to interference with this freedom of access, *e.g.* (1) the deposit or coating of grease or dirt on either side of the tube surfaces; (2) imperfect and partial, or sluggish circulation of either steam or condensing water, or both, over the surfaces; (3) water-drowning of the lower portions of the surface on the steam side, brought about by the partial flooding of the tubes with the water of condensation when finding its way to the bottom of the condenser; (4) air-drowning of the surface on the steam side by the pressure of air in with the steam, and especially by its accumulation in the lower parts of the condenser. Of these influences only the second and third can be obviated by condenser design and proportions, and they were the chief theme of the paper I had the honour of reading before the Institution two years ago. The fourth is the special subject of Mr. Morison's paper. In that paper it has been shown how the presence of air in a condenser interferes with condensation and lowers the vacuum; and the practical question is how it can best be dealt with. Clearly, the access of air to, and its accumulation in a condenser cannot be ascribed to the action of the condenser of itself. Whatever may be the reason for the air gaining access, its non-removal is at any rate due directly to insufficiency of the air-pump, which insufficiency reacts on the condenser performance in such a way as to intensify the general effect. The mere passage of air through a condenser has undoubtedly a certain influence in impeding condensation, but the vastly greater effect takes its rise from the accumulation of air in the condenser. The precise manner in which air degrades condenser performance is perhaps best understood when one considers what happens inside a condenser at work, in which air is permitted to accumulate. The primary cause of the accumulation is, of course, the inability of the air-pump to cope, at the then existing vacuum, with the quantity of air entering the system. Air being heavier than steam at all pressures obtaining inside a condenser, it will gravitate to the lower regions of the condenser, and will form an air-layer, the depth of which will increase until the pressure due to its weight and bulk rises to such an extent as enables the air-pump to extract per unit time the same weight as gains access. Then—and only then—will equilibrium be attained, and the condenser will now be working with a greater or less proportion of its surface drowned in air, to which surface the steam cannot penetrate. The immediate result is that the effective condensing surface is reduced, the condensation-rate on that portion of the surface which remains effective is increased, the temperature of

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condensation is raised, and the vacuum is lowered. The lowering of the vacuum is due to a twofold cause, the pressure of the accumulated air and the higher temperature of the steam, *i.e.*, vacuum is lowered more than temperature is raised, by the amount of the air pressure. Anything which increases the air-extracting capacity of the pump will lower the surface-level of the air layer, and so render effective a greater proportion of the condensing surface, with the resulting lowering of condensation-temperature and raising of vacuum.

A larger or a more efficient pump is an obvious preventive of air accumulation. Another remedy is by the use of a cooler to so lower the temperature of the steam water, that the air-extracting capacity of the pump may be raised without increasing the size of the pump itself. If there be absolutely no air in a condenser, clearly no air-pump is required, only a water-extracting pump; and in such conditions the resulting vacuum will depend solely upon the mean temperature obtaining in the condenser on the steam side of the surface. It will, therefore, be due to condenser action alone, and no cooling of the pump contents will affect the vacuum appreciably. In this case, also, the vacuum will, of course, be the highest possible in the conditions, and the particular relations which exist between the different water temperatures and the vacuum will depend upon the characteristics in respect of proportion and design adopted for the condenser, and will be quite independent of the air-pump capacity, so long as the latter is at least sufficient for removal of the steam water. When, however, air enters the system, and especially when the pump fails to prevent its accumulation inside, then the results are radically modified to an extent depending on the pressure the air attains. The greater the air-extracting capacity of the pump the less will be the departure, with given air leakage, from the no-air conditions just referred to. The precise effect—or rather series of effects—which various degrees of air leakage may have upon the performance of a condensing plant, can be ascertained only by actual experiment on such plants. Mr. Morison has very clearly shown in his paper how the air-extracting capability of a pump depends upon its temperature, and has referred to the influence which an independent steam-water cooler has in sustaining the vacuum, in spite of considerable air leakage.

From many careful experiments I have made with the cooling apparatus referred to, I can endorse to the full what he has said as to its action. When properly designed and fitted, it acts as a valuable auxiliary to the air-pump in enabling the latter to prevent such air accumulation as would interfere with the effective operation of the condenser surface in its necessary and proper work of condensation. Furthermore, I have found it to be automatic in its action in this respect, that when it is associated with a compartment-drained condenser, such as one of the "Contraflo" type, practically no heat is carried off from the feed by the condensing water when there is no air leakage. And only when the air accumulates to such a degree as to seriously threaten the vacuum, does the arrangement so adjust itself that, in conjunction with the air-pump action, the steam-water temperature is lowered and the air-extracting capacity of the pump is raised. In other words, heat loss occurs only when such loss becomes inevitable, as being the price which has to be paid for vacuum-maintenance in face of increased air leakage; and when there is no necessity to incur such loss the independent cooler system adjusts itself accordingly. As an illustration of the utility of the air indicator referred to in Mr. Morison's paper, Diagram III. (page 159) may be noted. In the course of experimenting I very often had occasion to notice that with reciprocating engines working at low power the vacuum was invariably much lower than it ought to have been, judging from the results yielded at higher powers. Internal evidence pointed to an increase of air as being the cause. But I had no independent and external evidence on the point until the air indicator was devised and applied. When this was done, it was visibly evident (see diagram) that air leakage grows in amount as the steam pressure in the low-pressure steam chest diminishes; and, of course, the vacuum falls in proportion, notwithstanding the fact that the rate of condensation per square foot is also falling. From the calibration of the instrument the figures in the base may be converted into cubic feet of air discharged by the air-pump per hour, and this also is given on the diagram.

There is only one other point that I should like to mention. We all know what is meant by

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"specialisation of function"; it is rather a formidable phrase, but we know that it applies in nature, and I think it applies in the mechanical world as well. When James Watt split up the steam cylinder previously existing into a steam cylinder and a separate condenser, he specialised the function of each of the two organs with a great gain thereby, and I am perfectly convinced that that is what you have to do in the case of a condenser. A condenser should be a condenser, and should condense steam and should not cool water; and, if you *must* cool water, you should cool it in a separate or independent cooler. Only then will you get the maximum amount of efficiency in either operation.

Mr. E. HALL-BROWN (Member): Mr. Chairman and Gentlemen, I wish to express my high appreciation of the paper which Mr. Morison has just read upon a subject which, as you are well aware, he has made particularly his own. We engineers who have been dealing with steam are sometimes likely to forget that an air-pump is meant to deal with air, and we have been thinking of it as an instrument which should have a certain relation, as regards its capacity, to the amount of steam being condensed. It is well, therefore, that we should be reminded that if only steam entered a condenser no air-pump would be required; and that the capacity of the air-pump has absolutely no relation to the amount of steam that is being condensed. It therefore follows that the practice of proportioning the capacity of an air-pump to the amount of steam to be condensed can only be correct in so far as the proportions of the mixture of air and steam are constant. I daresay many puzzling experiences that we have had would have been completely explained had we remembered that the amount of air leakage was a varying quantity in the average reciprocating engine, and for the lucid information regarding this point alone I should have welcomed Mr. Morison's paper. With regard to the air-pump itself, it is quite evident, especially now that it has been so clearly pointed out, that no air-pump can possibly discharge the water of condensation at a temperature as low as the theoretical temperature corresponding to the pressure within the condenser, because part of that pressure is due to the air within the condenser, and consequently the relation between the temperature of the air-pump discharge and the pressure within the condenser is no measure of the air-pump's efficiency. I think some of us have been treated to statements intended to show the efficiency of certain air-pumps by comparing the temperature of discharge from the air-pump with the theoretical temperature of the steam within the condenser. As a matter of fact, if the two temperatures at all approached each other, it was not the air-pump efficiency that was particularly good, but the air leakage that was particularly small. I think the crowning point in the whole paper is that Mr. Morison has pointed out a way in which the volumetric efficiency of the air-pump can be increased at the same time that the discharge from the condenser is maintained at a high temperature, and this result is obtained in a manner which makes a minimum drain on the amount of heat in the feed water. This, I think, is a very great achievement, and one which, even in the case of a reciprocating engine, is of very great value. In the case of turbine engines, where a high vacuum is essential to maximum economy, the value is still further increased; and, should Mr. Parsons' proposal of a combined system of reciprocating and turbine engines become universal in cargo boats and elsewhere, this system of Mr. Morison's should, I think, have a very great future before it.

Mr. LESLIE S. ROBERTSON (Member): Mr. Chairman and Gentlemen, I have not much to add to the discussion on this paper. The feeling that has impressed itself on my mind since I came into this room and heard Mr. Morison's lecture is, if I may put it in that way, rather that of realising how little we yet know about this subject; and I think we are particularly happy on this occasion in having you, Sir, the father of turbine work, in the chair, because, after all, the question of vacuum is far more important in turbine work than it is in reciprocating engine work. The paper in itself is extremely difficult to discuss. One of the neatest arrangements in the whole system put forward by Mr. Morison is, I think, the little air indicator, and for this reason: it will be generally admitted in engineering that, if we are to get higher efficiency out of any particular part of the machinery, or the machinery as a whole, the only way we can do so is to watch each individual detail, and Mr. Morison (in collaboration with Professor Weighton), by enabling a quantitative measurement to be taken of the actual air passing

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through the condenser, has made it possible for those in charge of steam plant to watch the air leakage in a way they could never do before. I had the opportunity of seeing this little instrument at work on a Parsons turbine at Hartlepool, and it was of great value in checking the exact amount of air leakage that was taking place. I am sure that the importance of having such an instrument will be better realised as we recognise the fact that the loss of power in a turbine or other engine due to bad vacuum goes on night and day; and, although it may seem trifling, perhaps, at first, I feel quite sure that engineers will get a better return from their plant by being able to watch the details as they go along. By thus enabling the engineer easily and effectually to locate trouble, and the exact amount thereof, Mr. Morison has, I consider, rendered a valuable service to steam engineering. There is only one other point to which I should like to allude, and that is the flat portion of the curve in Diagram III. In all engineering it is a good thing to have something up our sleeve, and this is particularly so in the case of the vacuum. If one really appreciates what the horizontal position of that curve means in conjunction with the employment of the cooler, one realises what an advantage is obtained therefrom in everyday practical working.

Mr. R. J. WALKER (Member): Mr. Chairman and Gentlemen, the theoretical treatment of the subject of this paper by Mr. Morison is undoubtedly very interesting, and the models we have seen are especially so. From the early days of turbine development the desirability of obtaining a high vacuum has been very fully recognised, and many experiments were carried out with condenser apparatus by the Parsons Companies, which subsequently led to the fitting of a vacuum augmentor for reducing the air in condensers to an absolute minimum. The augmentor is equivalent to an air-pump of very large volumetric capacity, and the effect of extracting the air is immediately seen in the high rate of condensation obtainable with its use. On page 169 will be found in tabulated form several results which have been obtained by the use of this vacuum augmentor. The first example in the Table, of a land turbine installation, is, I think, an excellent result. With a temperature of inlet cooling water as high as 82 degrees and an outlet of 94 degrees, the vacuum is 28·21 inches. This vacuum was obtained when condensing 10·63 lbs. of steam per square foot of cooling surface. In another case, with an inlet temperature of 52 degrees and outlet 66·7, a vacuum of 29·22 was obtained when condensing 11¼ lbs. of steam. In marine work the augmentor has also given excellent results. In the case of the *Manxman*, with an inlet temperature of 54 degrees, and outlet of 83, a vacuum of 28·65 was obtained when condensing 12 lbs. of steam per square foot of cooling surface. Other results can be seen by reference to the table. Mr. Morison has rightly shown the necessity of a low temperature of condensed water corresponding to a high vacuum; but I think, if I remember correctly, that in the paper referred to as being read by Professor Weighton (in 1906) it was mentioned that a high hot-well temperature could be obtained in conjunction with high vacuum. As far as I can see, I am afraid it is rather difficult to have it both ways.

Mr. D. B. MORISON (Member of Council): Mr. Parsons and Gentlemen, I am exceedingly obliged for the interest you have taken in the paper I have submitted, and particularly to those members who have contributed to the discussion. Professor Weighton draws attention to the fact that, whilst the passage of air through a condenser has a prejudicial effect on condensation, yet the vastly greater effect results from the accumulation of air in the condenser. It is not so much the air which passes *through* a condenser, but the air which remains *in* a condenser which is the cause of surface inefficiency, so that excellence of air-pump, rather than mere size, is the main essential. I quite agree with Professor Weighton with regard to the importance of steam flow over the condensing surface, and I have found the tubeless passages, which are a distinguishing feature in the "Contraflo" system, very effective in this respect. These passages form collecting and distributing chambers in which the steam changes its direction of flow when passing from one nest or section of tubes into the succeeding nest, a practically uniform velocity of flow being maintained over the entire tube surface. Fig. 9 (Plate XVI.) shows an arrangement with these tubeless passages at the sides and in the centre, the object and technical effect in all cases being uniform distribution over the surface and the maintenance of definite and uniform

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TEMPERATURES FROM VARIOUS AUGMENTOR CONDENSERS IN ACTUAL PRACTICE.

On Land.

Machine No.	Destination.	Cooling Water. t_1 °F. (Inlet)	t_2 °F. (Outlet)	$t_2 - t_1$	Lbs. of Cooling Water Per sq. ft. tube Surface.	Velocity ft. Per Sec.	Temp. of Steam Space °F.	Condenser. Va. at 30 in. Bar.	Abs. Press. in ins. H_g.	Temp. Corresponding to Vac. °F.	$T - t_1$ °F.	Conductivity per °F. difference across the tubes.	$\frac{t_2 - t_1}{T - t_1}$	Air Pump Discharge. Temp. °F.	Lbs. of Water per Hour. Total.	Per sq. ft. of Surface.	Sq. ft. Surface.
1,102	St. Marylebone Electric Works, London.	82·4	94·9	12·5	843	7·4	102·8	28·21	1·79	97·68	15·3	1,525	·823	96·4	35,553	10·63	3,350
1,102		76	77	1·0	1,090	9·1	—	29·06	·94	77·3	1·3	—	·77	83·5	3,650	1·09	3,350
1,044	Newcastle & District Electric Light Co., Newcastle-on-T.	52·4	66·7	14·3	785	8·6	75·7	29·22	·78	72	19·6	1,083	·73	68·75	24,730	11·25	2,205
1,044		51·8	57·4	5·6	684	7·7	61	29·52	·48	58·0	6·2	1,725	·905	64·6	8,500	3·86	2,205
1,091	Alexander Pirie & Son, Bucksburn, Aberdeenshire.	56·7	64·0	7·3	855	7·74	72	29·37	·63	65·5	8·8	1,612	·83	71	4,428	6·23	710
1,091		56·0	63·5	7·5	623	5·6	75	29·34	·66	67	13	712	·682	72·5	3,320	4·67	710
1,007	St. Marylebone Electric Works, London.	76·5	89·5	13	846	7·05	100	28·33	1·67	95·3	18·8	985	·692	89·76	35,155	11·0	3,200
1,008		72·8	86·8	14	742	6·22	98	28·39	1·61	94·1	21·3	790	·657	87	34,833	10·4	3,350
991	Manchester Corporation Electric Works.	92·8	111·3	18·5	475	6·66	119	26·77	3·23	117·8	25	655	·74	110·1	15,800	8·77	1,800
1,048	Kent Electric Power Co., Ltd.	67	74	7·0	217	1·82	89	29·12	·88	75·3	8·3	—	·844	88·5	4,550	1·52	3.000
1,059	Great Eastern Railway Co.	80·2	95	14·8	782	6·83	106	27·87	2·13	103	22·8	—	·648	103·5	17,340	11·55	1,500
1,126	Ocean Coal Co., Ltd., Cardiff.	45·5	49·9	4·4	299	3·52	78	29·61	·38	52	6·5	—	·676	58·5	2,200	1·32	1,670
	Readings Taken on Marine Condensers.																
	Passenger Steamer *Manxman.* (Fitted with Augmentor.)	54	66	12	383	3·5	—	29·3	·7	68	14	757	·857	—	39,000	4·6	8,500 (2 cond.)
		54	76	22	390	3·6	—	29·0	1·0	79	25	825	·88	—	73,000	8·6	„ „
		54	83	29	415	3·8	—	28·65	1·35	89	35	730	·83	—	102,000	12·0	„ „
		54	89	35	410	3·75	—	28·35	1·65	95	41	800	·855	—	124,000	14·6	„ „
		54	87·5	33·5	515	4·7	—	28·0	2·0	101	47	650	·714	—	146,000	17·2	„ „
	Turbine Yacht. (Fitted with Augmentor.)	44	73	29	488	—	91·5	28·95	1·05	81·5	37·5	730	·775	78	82,000	14·11	5,800 „
		44	70	26	278	—	82·0	29·2	·8	72·5	28·5	670	·915	—	42,000	7·25	„ „
	Third Class Cruiser. (No Augmentor.)	51	96	45	420	3·8	117·0	26·6	3·4	120	69·0	444	·65	79·9	190,525	18·85	10,100 „
		52	83·6	31·6	316	2·9	102	27·9	2·1	103	51	306	·62	80·4	100,600	10·0	„ „
		52	87·4	35·4	214	1·97	111	26·8	3·2	118	66	162	·535	79·7	76,490	7·57	„ „
		52	76·6	24·6	177	1·62	111	26·6	3·4	120	68	87	·39	81·2	44,090	4·36	„ „

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flow towards, and finally into, the air-pump. What Professor Weighton aptly refers to as "specialisation of function" is illustrated in Fig. 10, Plate XVI., which represents in diagrammatic form a condensing plant for a land installation. The pumps are of the three-throw arrangement, one pump drawing away the condensed steam water, and the other two the air. The working of the hot water pump is not, in any way, dependent on temperature. It withdraws the water of condensation as hot as the condenser can deliver it, thereby effecting maximum available thermal efficiency. The efficiency of an air-pump depends, however, on the temperature of the water passing into it. and the lower that temperature relatively to the vacuum temperature the more efficient is the pump. The bottom of the condenser is, therefore, arranged to cool water passing through the air-pumps in continuous circulation, and the small amount of surplus water represented by the vapour which passes into these air-pumps, and is therein condensed, overflows amongst the feed water discharged by the water pump. By this arrangement maximum thermal efficiency is secured, together with high air-pump efficiency; moreover, the water supply to the air-pumps is in the form of a constant flow, and of a quantity best adapted for their satisfactory working.

Mr. Hall-Brown's remarks are a very clear reply to a question I am often asked, as to why feed water can, with advantage, be deliberately cooled. The object, of course, is to cool the water, not as much as possible, but as little as necessary, the degree of cooling required depending on the ability of the air-pump to deal with any given air leakage; hence the value of having the cooling under control. In an ordinary condenser the cooling is out of control, as a drop of water formed on the top tube falls over tube after tube until it reaches the bottom of the condenser at the lowest temperature the conditions will permit. If this temperature is lower than the pump requires in order to effectively rarify the condenser, the result is needless and unnecessary thermal loss; whereas, if the temperature can be controlled and adjusted to the requirements of the air-pump, the condenser efficiency will be maintained with a minimum thermal loss. Mr. Leslie Robertson refers to Professor Weighton's air gauge. I have tried various modifications of this apparatus, and have found the design shown in Fig. 11 (Plate XVI.) very satisfactory and reliable. The air discharge pipe from the air-pump is normally closed by a non-return valve. The air is led to the air indicator, which is conveniently placed in the engine room. The indicator consists of an inverted bell centrally guided within a glass cylinder containing water. Air passes to the interior of the bell and escapes therefrom by one or more holes in its side. The bell is fitted with a buoyancy chamber, and when not in action it floats so that the uppermost air escape hole is just below the water level. On air being admitted, the bell rises until the number of holes which rise above the water level are sufficient to permit the air to freely escape without perceptible increase of air pressure; and as the quantity of air passing through any number of holes is previously ascertained by calibration, the cubic feet of free air passing through the system is always in evidence. This apparatus is extremely valuable in connection with condensing plant, as the engineer can ascertain the air leakage at all times, and by reducing it to a minimum he will not only minimise the power required by the air-pump—which, as Mr. Leslie Robertson rightly says, goes on day in and day out—but he will maintain the condenser as free from the evil effects of air as the particular air-pump employed will permit.

Mr. Walker describes Mr. Parsons' clever invention of the vacuum augmentor as being equivalent to an air-pump of very large volumetric capacity. I quite agree, but it should not be lost sight of that the power absorbed by it is proportionately great; and whereas, with an air-pump of any ordinary type the steam necessary to drive it decreases with every decrease in air leakage, the steam required by an augmentor is practically a constant quantity, and therefore is of extravagant amount with small air leakage. In a recent test of a Contraflo plant of 10,000 H.P. at Manchester, the power absorbed by the air-pump, with a slight air leakage, was a quarter of 1 per cent. of the full load power—a result quite impossible to obtain with an augmentor, which is essentially an apparatus for dealing with heavy air leakage. An instructive feature in Mr. Walker's table is that which shows the result obtainable with an abnormal quantity of circulating water relatively to the steam condensed. For example, the second test at Marylebone with machine No. 1,102 shows over 1,000 times water, Newcastle

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second test 178 times, Kent 143 times, and Ocean Coast Company 227 times. These results, of course, were obtained under light loads, and are, therefore, of scientific rather than commercial interest. Marylebone, under normal load, with 85 times water and a velocity of 7·4 ft. per second, through the tubes, illustrates what can be done when conditions permit of an exceptionally large power being absorbed by the circulating pump; but even with turbines there is, of course, a combined power of air and circulating pumps beyond which it is commercially unprofitable to go. With regard to Mr. Walker's reference to the temperature of the water of condensation, my reply is that in a suitably arranged and proportioned condenser, with minimum flooding of the tubes and minimum air, the temperature of the condensed steam water will, with an efficient air-pump, closely approach the vacuum temperature at any vacuum within the ordinary range.

The CHAIRMAN (the Hon. C. A. Parsons, C.B., M.A., F.R.S., D.Sc., Member of Council): Gentlemen, I am sure it is your wish that we should thank Mr. Morison for his interesting paper on condensers, a subject which is of special importance in relation to turbines where the very highest vacuum is most desirable.

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NOTE ON THE USE OF SUPERHEATED STEAM WITH MARINE ENGINES.

By Monsieur FÉLIX F. T. GODARD, Member.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 9, 1908 ; The Hon. C. A. PARSONS, C.B., M.A., F.R.S., D.Sc., Member of Council, in the Chair.]

SUPERHEATED steam was used in marine engines more than half a century ago, after Hirn's noteworthy experiments with the "Logelbach" engine in Alsace. The French Navy also tried it on some of their earliest protected cruisers.

These early attempts were not, however, followed up, as it was found difficult to construct superheaters capable of maintaining a constant and sufficiently high temperature, and also because of the wear and tear of the hemp packings in use at that period.

The introduction of compound marine engines, more economical than the simple engines that had preceded them, caused the use of superheated steam to be given up for the time being. The same thing occurred with stationary engines, where improvements in valve gear enabled a high ratio of expansion to be employed and the clearances to be reduced to a very small percentage of the cylinder volume.

In the Vosges and Alsace, however, the problem of using superheated steam in stationary engines was revived some fifteen years ago. Several different arrangements were designed by Mr. E. Schwoerer, a former assistant of Hirn's, who used a massive superheater placed behind the fire bridge of the boiler furnace, and this gave such promising results that the study of the question of superheated steam was taken up by a number of manufacturers, chiefly in Germany, Alsace, and Switzerland.

It was found that engines fitted with Sulzer Colman valves, which were largely employed in those countries, were very suitable for use with superheated steam. In France, where the Corliss gear was usual in stationary engines, superheating did not make much progress, because it was not suited to the Corliss engine, or, in fact, to any flat slide-valve engine. The exhibits at the Paris Exhibition of 1900 showed this to be the case.

Since then the use of superheated steam with stationary engines has increased largely, and considerable economy has been effected thereby. It is not unusual to

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find engines of 1,500 I.H.P. to 2,000 I.H.P. using steam at 300° C. (572° *Fahr.*) and working at an expenditure of only 4 kgms. (9 *lbs.*) of steam per I.H.P.* per hour.

There is but little information, however, on the subject of the variation in the consumption of steam in relation to its temperature. A few years ago the author made some experiments on a triple-expansion engine with piston valves, the degree of superheat varying from 0 to 120° C. The results of these trials still present some features of interest. In the accompanying diagram (Fig. 1) the abscissæ represent the amount of superheat, *i.e.*, the difference between the actual temperature of the steam (in degrees C.) and the temperature corresponding to the pressure when the steam is saturated. The ordinates represent the weight of steam consumed per effective brake horse-power.

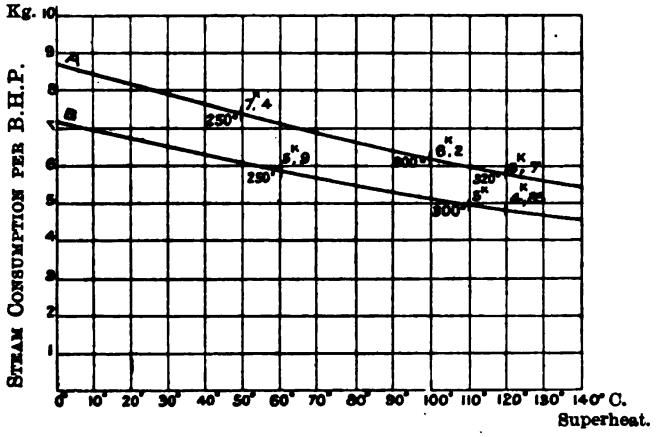
TRIPLE-EXPANSION ENGINE.

Diameter of cylinder (H.P.)	0·20 m. ($7\frac{7}{8}$ *in.*)
„ „ (M.P.)	0·33 m. (13 *in.*)
„ „ (L.P.) 2 of	0·37 m. ($14\frac{9}{16}$ *in.*)
Stroke	0·29 m. ($11\frac{7}{16}$ *in.*)
Revolutions	440
Power	300 B.H.P.
Cut-off	0·7
Ratio of expansion	9·8
Pressure in main steam pipe (A)	12·8 kgms. (182 *lbs. per sq. in.*)
„ „ „ (B)	15·0 kgms. (214 *lbs. per sq. in.*)

A exhaust to atmosphere.
B exhaust to condenser.

Now, in the diagram (Fig. 1) curve A (exhaust to atmosphere) shows that the consumption per hour of saturated steam (*i.e.*, with no superheat) is 8·85 kgms. ($19\frac{1}{2}$ *lbs.*) per B.H.P., whereas it falls to 5·70 kgms. ($12\frac{3}{4}$ *lbs.*) at a temperature of 320° C. (608° *Fahr.*), equivalent to a superheat of 120° C. (216° *Fahr.*).

FIG. 1.



The saving therefore amounts to—

$$\frac{8{\cdot}85 - 5{\cdot}70}{8{\cdot}85} = 35{\cdot}5 \text{ per cent.}$$

Taking curve B (exhaust to condenser), the consumption per B.H.P. falls from 7·15 kgms. (16 *lbs.*) with no superheat, to 4·85 kgms. ($10\frac{1}{2}$ *lbs.*) with a superheat of 120° C., or a saving of—

$$\frac{7{\cdot}15 - 4{\cdot}85}{7{\cdot}15} = 32 \text{ per cent.}$$

* I H.P. referred to in this paper is French "force de cheval" of 75 kgms. = ·986 British I.H.P.

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It will be seen, therefore, that superheating may lead to an economy, as compared with saturated steam, of 35 per cent. in engines of this type exhausting to the atmosphere, and 32 per cent. for those exhausting to the condenser.

The amount of reduction in steam consumption depends, of course, upon the design of the engine under consideration; in the present case it amounts to about 1 per cent. for every 4° C. (7·2° *Fahr.*) of superheat. This is a figure frequently given, and which the author has been able to verify elsewhere.

Doubts, however, have often been expressed in regard to the efficiency of superheating in actual practice. These arise from the wear of the valve gear of the engines, which causes losses that neutralise part of the economy obtained by using superheated steam.

It is now recognised by all the makers of land engines that, in order to use steam at a temperature of 280° to 320° C. (536° *to* 608° *Fahr.*), the engine friction must be as small as possible, as, for instance, in reciprocating engines with lift valves, and still more so in turbine engines.

Superheated steam is now generally adopted for land engines by reason of the economical results obtained in practice, which, in certain special cases, have effected a saving of upwards of 33 per cent.

For marine engines the case is very different. England was the first country to take up the matter; in 1900 Messrs. Wilson & Sons, of Hull, installed superheaters on board the S.S. *Claro*, which appear to have given satisfactory results. Other installations followed with varying measures of success. The British Admiralty investigated the question on the *Britannia* with satisfactory results.

Superheating has also been adopted in the United States on the S.S. *Creole* fitted with Curtis turbines, and in Germany on the *Ersatz Komet* with Parsons turbines. Nevertheless, superheating in marine engines cannot be said to have gained ground as rapidly as was expected. It may, therefore, be of interest to record some very encouraging results which have been obtained in France within these last few years.

In 1906 the Société de Saint-Nazaire built two identical cargo boats for the Compagnie Générale Transatlantique. They were the *Garonne*, fitted with ordinary triple-expansion engines and slide valves, and the *Rance* with similar engines, but with the Lentz valve gear. The boilers of the latter vessel are similar to those of the former, except that they are fitted with Pielock superheaters.

The leading dimensions of these two ships, their engines, and boilers are as follows:—

Length	91 m. (298 *ft.* 6 *in.*)
Beam	12·20 m. (40 *ft.* 0 *in.*)
Moulded depth	7·75 m. (25 *ft.* 5 *in.*)
Load draught	6·40 m. (21 *ft.* 0 *in.*)
Gross tonnage	2,700 tons.

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ENGINES.

Diameter of cylinders (H.P.)	0·584 m. (23 *in.*)
„ „ (M.P.)	0·914 m. (36 *in.*)
„ „ (L.P.)	1·498 m. (59 *in.*)
Stroke	1·066 m. (42 *in.*)

The boiler installation of each ship consists of two cylindrical boilers, each having two furnaces and fitted with Howden's forced draught.

	Garonne.	*Rance.*
Total grate area—sq. m.	8·40 (90·42 *sq. ft.*)	8·40 (90·42 *sq. ft.*)
Heating surface—sq. m.	350·08 (3,767 *sq. ft.*)	277·08 (2,982 *sq. ft.*)
Superheating surface–sq.m.		73·00 (785 *sq. ft.*)
Total heating surface–sq.m.	350·08 (3,767 *sq. ft.*)	350·08 (3,767 *sq. ft.*)

The trials of these two vessels were carried out under conditions as similar as possible, so that the comparison of results might be quite fair, and the coal used was the same in both cases. The results of the trials were as follows :—

COAL CONSUMPTION TRIALS.

	Garonne.	*Rance.*
Date of trials	July 6, 1906.	Sept. 13, 1906.
Boiler pressure	12·60 kgms. (178 *lbs. per sq. in.*)	12·54 (177 *lbs. per sq. in.*)
Steam temperature	192° C. (377·6° *Fahr.*)	270° C. (518° *Fahr.*)
Revolutions	72·3	75·37
I.H.P.	1,104	1,304
Coal consumption—per I.H.P. ...	511 gr. (1·12 *lb.*)	408 gr. (0·9 *lb.*)
Advantage of Superheating { Increase of power		18·1 per cent.
Advantage of Superheating { Reduction of coal consumption		20·1 „

Both ships were put into service directly after the trials. It is now over a year since these two cargo boats have been engaged upon an exactly similar service, and it has been possible, therefore, to obtain accurate data regarding their working and comparative coal consumption.

Taking for each ship ten trips made at corresponding dates, so as to have as far as possible identical conditions of weather, loading, and quality of coal used, the fuel consumption per mile worked out at 69·981 kgms. (154 *lbs.*) for the *Garonne* without superheating ; 57·228 kgms. (126 *lbs.*) for the *Rance* with superheating.

Comparing these figures we have an economy in coal consumption in favour of the *Rance* of—

$$\frac{69{\cdot}981 - 57{\cdot}228}{69{\cdot}981} = 18{\cdot}2 \text{ per cent.}$$

There was, moreover, no trouble with either engines or boilers. No leakage has occurred in the valves, which continue to bear simultaneously on both

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upper and lower seatings. The superheater has not required any particular attention; a constant steam temperature has been maintained, which rises only a few degrees when additional power is required of the boilers, and falls again automatically directly the engines are stopped.

In consequence of the results of the early trials of the *Rance*, the Compagnie Générale Transatlantique installed Pielock superheaters and Lentz valve gears on the S.S. *Pérou*, for service to the Antilles (West Indies), and also on the intermediate cargo boat *Caroline*. The same arrangement is being adopted on the cargo boat *Honduras*, and other cases are under consideration.

Each of these vessels is to be comparable to a sister ship, working with saturated steam, in order to follow up on a larger scale the conclusive results already obtained with the *Rance* and the *Garonne*.

The S.S. *Pérou* has just completed her trials. She is identical, save for the superheating and valve gear, with the S.S. *Guadeloupe*, employed on the same service, and which was completed in September, 1907.

The dimensions of these two ships are as follows:—

Length	131 met.	(429 *ft.* 9 *in.*)
Beam	15·86 „	(52 *ft.*)
Moulded depth	10·50 „	(34 *ft.* 6 *in.*)
Load draught	6·60 „	(21 *ft.* 7 *in.*)
Gross tonnage	6,800 tons.	

Each vessel is fitted with twin-screw triple-expansion three-cylinder engines of the following dimensions:—

	Guadeloupe.	*Pérou.*
Diameter of cylinders (H.P.)	0·685 m. (27 *in.*)	0·685 m. (27 *in.*)
„ „ (M.P.)	1·092 m. (43 *in.*)	1·060 m. (41¾ *in.*)
„ „ (L.P.)	1·828 m. (72 *in.*)	1·828 m. (72 *in.*)
Stroke	1·219 m. (48 *in.*)	1·219 m. (48 *in.*)

The illustrations on Plate XVII. give the details of the valve gear and a section of the boiler and superheater.

The boiler installation of each ship comprises six cylindrical boilers having three furnaces each, and fitted with Howden's forced draught.

	Guadeloupe.	*Pérou.*
Total grate area—sq. m.	32·130 (346 *sq. ft.*)	32·130 (346 *sq. ft.*)
Boiler heating surface—sq. m.	1,255 (13,509 *sq. ft.*)	932·70 (10,030 *sq. ft.*)
Superheater heating surface—sq. m.		302·00 (3,260 *sq. ft.*)
Total heating surface sq. m.	1,255 (13,509 *sq. ft.*)	1,234·70 (13,290 *sq. ft.*)
Working pressure	13 kgm. (185 *lbs. per sq. in.*)	

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The speed trials of both vessels, which were carried out under absolutely similar weather conditions, gave the following results :—

	Guadeloupe.	*Pérou.*
Date of trials	Sept. 9, 1907	Feb. 6, 1908.
Steam	Saturated	Superheated
Boiler pressure	13 kgms. (185 *lbs. per sq. in.*)	13 kgms. (185 *lbs. per sq. in.*)
Temperature of steam at engines	192° C. (377·6° *Fahr.*)	238° C. (460° *Fahr.*)
Revolutions	88·19	88·47
I.H.P.	6,585	6,750
Speed	16·60 knots	16·95

(or a gain of 0·35 knot in favour of S.S. *Pérou*).

These results were considered most satisfactory both by the Postal Commission and by the owners of the vessels.

A noteworthy feature is the constant temperature of the superheated steam. This temperature was taken at admission of steam to the engines by means of a

FIG. 2.

TEMPERATURE OF SUPERHEATER ON SPEED TRIAL OF 6TH FEBRUARY, 1908.

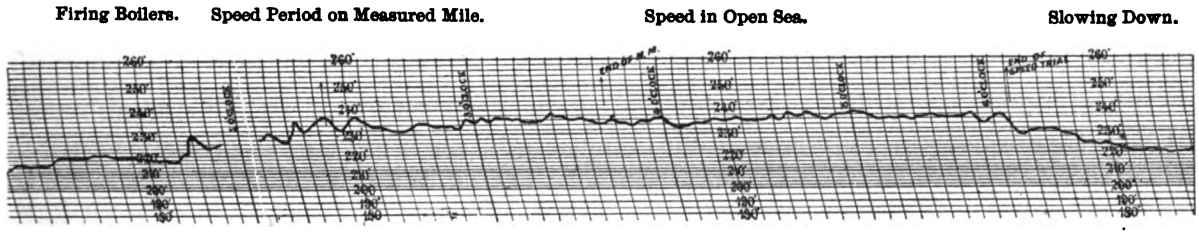


FIG. 3.

CURVES OF TEMPERATURE ON REGULAR SERVICE.

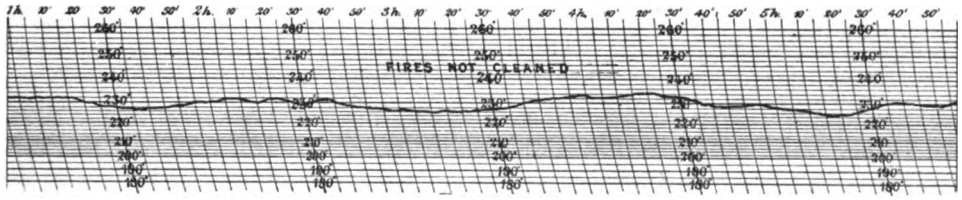
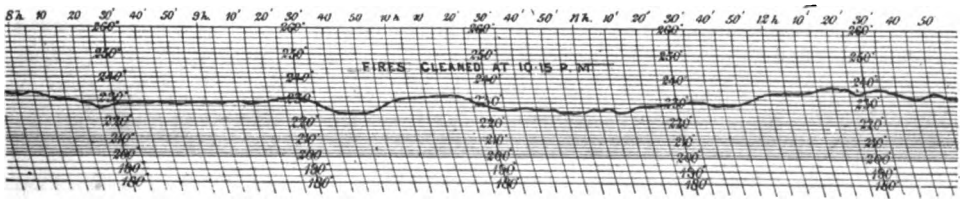


FIG. 4.

CURVES OF TEMPERATURE ON REGULAR SERVICE.



Fournier recording thermometer. The diagram (Fig. 2) represents the temperatures of the steam, both during firing of the boilers before the trials, during the trials,

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and also while slowing down. The diagrams (Figs. 3 and 4) were taken during the first voyage in service. They represent an interval of one watch, which covers, a period of normal steaming and a period of cleaning fires.

It will be seen that the variations of temperature are very small, and do not exceed 20° C. (36° Fahr.) from the time of starting the engines to that of running at full power, which includes also the period of cleaning the fires.

These results are very interesting as being applicable also to the use of superheated steam in large turbines on board ship. The absence of sudden changes of steam temperature in turbine engines will prevent the apparatus from being exposed to sudden expansion and contraction, the effects of which might be serious.

From the results obtained both in England and in France, superheated steam applied to reciprocating engines conduces to considerable economy without the introduction of complicated machinery necessitating additional attention. There appears to be no reason, moreover, why superheaters of a type similar to that installed on the S.S. *Pérou* should not be fitted to the large turbines of ocean liners, using only a moderate amount, say, 60° to 70° C. (108° to 126° Fahr.), of superheat. From the experience gained with steam turbines on land, at least 10 to 12 per cent. fuel economy should result from this, being 1 per cent. saving for every 6° C. (11° Fahr.) of superheat.

Shipbuilders and shipowners may well turn their attention to this question, as it is likely to remove the only remaining objections to the general use of steam turbines for ocean navigation—a hope which is likely to be fulfilled at no distant date.

DISCUSSION.

Mr. JAMES H. ROSENTHAL (Associate): Mr. Chairman and Gentlemen, I think we are very much indebted to Monsieur Godard for giving us the results he has obtained with superheating. The question of superheating is one which not only affects the use of the steam turbines, but is of particular importance to the shipowner because he has, with the introduction of superheaters, a means at hand at a relatively low cost, of very largely increasing the economy of his vessel and saving coal, which, considering its present high cost and possible increase, is a point of great importance. The figures given in the paper correspond fully to the experience that I have had with hundreds of superheaters in land practice. Of course, the economy of superheating on board ship goes back more years than I can remember, and was proved long ago. The chief difficulties that were found when superheating was first introduced on board ship were mainly mechanical difficulties, but of one thing I am absolutely certain, and that is, if superheaters are to be used on board ship, you must have condensers better constructed than we usually find them, at any rate on merchant ships. I believe that Monsieur Godard has Parsons turbines under construction in which he proposes to use superheated steam, and whilst I fully appreciate our Chairman's conservatism

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with regard to the use of superheaters in connection with turbines to start with, still, I am quite sure that whatever difficulties, if any really do exist, he will be easily able to overcome them. The difficulties of variation in temperature that exist with superheating in electric light stations or in power stations will not be experienced on board ship. I have frequently observed that in electricity works, where the load falls rapidly and to a very great degree, the temperature does vary a good deal; but I have seen other cases in which boilers fitted with superheaters serve mill engines when the load is pretty nearly constant, and they run a whole day with only about 10° Fahr. difference of temperature. Besides, it is a very simple matter, by mixing superheated and saturated steam, to get any temperature that may be required. The superheat on H.M.S. *Britannia* has, of course, been very carefully investigated, and it is very easy to do so on a warship having several identical boiler compartments, by means of which trials with and without superheaters can be made. The economy that has been so obtained on H.M.S. *Britannia* corresponds entirely to the results obtained in the French ships. The question of the durability of the superheaters in marine work is often raised. In stationary or land practice it is now well known that, with reasonable treatment, superheaters will last as long as the boiler. Not long ago, my firm put before the Institution of Civil Engineers pieces of tube that had been in use night and day for seven or eight years, and which showed no appreciable wear at all. As far as the design of the superheater that has been fitted on the *Pérou* is concerned, I am surprised that it has run for a year without giving trouble. Having had a very large experience in superheating, I consider the type of superheaters that Mr. Hyde fitted on the S.S. *Claro* infinitely superior to and more reliable than I should judge this one to be. Only time will tell whether this type will answer or not, as they have only been in use a year so far. Probably the work that Mr. Hyde did was an inducement to these French engineers to adopt superheating on merchant ships as extensively as has apparently been done, and I hope it will not be long before we shall have as many vessels so fitted in the British Mercantile Marine.

Professor W. H. Watkinson (Associate): Mr. Chairman and Gentlemen, I notice on page 172, in the last paragraph but one, that reference is made to the necessity for special valves. It is said that Corliss and flat slide valves are not suitable when superheated steam is used. That may be so in some cases, but I have had experience of cases where Corliss valves have worked without the slightest trouble with steam superheated by about 150° Fahr. When one of these engines, of about 1,200 horse-power, was started there was a little trouble, due to the increased expansion of the long valve; but when a little had been turned off the ends so as to allow more freedom, the difficulties disappeared, and those engines have now been working, I think, for seven or eight years without any trouble and with a great increase in efficiency. Then, again, I know of many cases in which flat slide valves are working with perfectly satisfactory results with superheated steam, but these valves are provided with relief frames. I fully agree that for the highest superheating drop-valves or their equivalents should be used, but for ordinary superheating—say to 150° Fahr.—under proper conditions and with a good lubricant, flat valves will do very well. In some cases I have known, the trouble has not been with the valves at all, but with the piston rings. In one case that I have in mind, although everything else worked in a perfectly satisfactory manner, there was trouble time after time, through voyage after voyage, due to the breaking of the piston rings. On two successive voyages across the Atlantic two sets of piston rings broke, although in that case the amount of superheat was only about 150° Fahr. Until recently I had no satisfactory theory as to the cause of the trouble in that case. I imagined that it was due to bad design and workmanship in connection with the high-pressure cylinder, its piston, and its rings, and possibly these may have been partly responsible for the trouble; but I have learned quite recently of another possible cause. Some experiments have been made in my laboratory in connection with the effect of superheated steam on oil, and it has been found that with some kinds of oil a sticky residue is always left when superheated steam is used. Now, if a little dirt is carried into the cylinder, it will adhere to this sticky residue, and its accumulation within the cylinder will cause rapid wear of the cylinder and of the piston rings. I do not know whether this fully accounts for the undue wear which has been observed in some cases or not, but it

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does help, to my mind, to account for some of the difficulties which have been encountered. The figures regarding saving in the consumption of coal are such as have been frequently obtained, but I would not like it to be generally given out as a possibility that a saving of 35 or 33, or even 30, per cent. of coal will in all cases follow the use of superheated steam.

The CHAIRMAN: It is not coal, it is steam.

Professor WATKINSON: The saving in steam in most cases ranges from 15 to 25 per cent., and the saving in coal is not very different from that; but it depends, of course, on the efficiency of the superheater. I share, with Mr. Rosenthal, doubts regarding the durability of the type of superheater described in the paper. When steam is not flowing through this superheater, portions of the tubes within the tube plates of the superheater will become overheated, and they may possibly be made red hot, and I should anticipate very considerable difficulties, both due to the overheating of the tubes at that part, and due to leakage past the tube plates. I think Monsieur Godard has done good service by bringing this important subject before the Institution, and I beg to add my thanks to him for it.

Mr. H. WHEATLEY RIDSDALE (Member): Mr. Chairman and Gentlemen, I would like to say a few words on this paper, because I have had the opportunity of seeing two of the ships to which these superheaters have been fitted. As regards the particular type of superheater described in the paper, I think some of the previous speakers, finding it new to them, have not dealt quite fairly with all the points that present themselves to anyone who studies it rather more closely. It is found in practice that the difficulty of leakage does not really arise. Such leakage, if it existed, would be past the tubes where they pass through the tube plates of the superheating chamber. The pressure on the two sides of these plates is practically the same; the difference simply amounts to the drop in pressure, which, in any case, must be small, in the collecting pipe which leads the steam into the superheater. It would not be, I suppose, more than a pound or two per square inch at the outside. The leakage, therefore, could only be very small, indeed, and, if any does take place, the water that passes into the superheater is immediately vaporised by the high temperature reigning there. Then, I think, it must occur to everybody that this superheater is a thing which, once built into the boiler, must stay there, unless you break up the boiler to get it out. When, however, one thinks a little further, one sees that the only parts of the superheater which are really subject to wear and tear are the tubes, and that they are easily renewable.

I think it is a pity that in this paper we really have only records of fairly continuous conditions, first accelerating, then a period of steady speed, and then a deceleration. What I should very much prefer to see would be records taken during sudden stops and starts in manœuvres. It may be that, when a ship is going full speed and everything is in good working order, for some reason quite unconnected with the engine-room, the telegraph bell rings, and you have to stop. It happened to me once when, going on a measured mile. What had happened then was that the steering gear had gone wrong, and I had to shut off steam with the fires roaring away. In the case of a superheater of the sort under discussion the temperature inside the superheater would instantly become enormous, as one of the previous speakers has said, and probably the tubes would become red hot, and if one then turned steam on again to the engine, one would get what might be described as "very hot stuff." Of this sort of thing we do not get a record in the very regular curves of temperature shown under the conditions in which the measurements of temperature were made. I asked Monsieur Godard to make an experiment of that sort with one of these ships at the first opportunity he had, and he did not display very great alacrity, but I still hope he will do it. The question of temperature with the Pielock superheater seems to be of extra importance, because of the fact that it is nearly impossible to mix saturated steam with the steam coming from this apparatus, unless you practically reduplicate the whole of your lines of pipes, because the arrangement must be worked from the engine-room. Therefore you would have to bring *both* superheated and saturated steam all this way from the boilers to the engine-room. In the comparatively small ships which have been fitted hitherto, and which have only one or two boiler-rooms, that is not, perhaps, prohibitive, but in the case of long ranges of steam pipes this would become a matter of very serious difficulty.

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It appears from the experience in France that engines with what we call "drop valves" are particularly necessary with considerably superheated steam. If this is the case, it seems to limit very much the application of superheated steam to reciprocating marine engines. As to these two ships, for instance, in one case the revolutions were 75, in the *Rance*, while in the *Pérou* they were 88, and the drop valves appeared to work exceedingly well under those conditions. I happen to know, however, that in France the experiment with drop valves has been carried a good deal further, and these Lentz valves have been fitted to the engines of a small torpedo boat running at over 300 revolutions per minute. Those engines have been undergoing trials over a period now approaching a year, and are not through their troubles yet. If, therefore, this system is to be limited to the slow-running engines of cargo boats, the matter becomes of considerably less interest than it otherwise would be. I should just like to correct one remark made by a previous speaker. He stated, I think, that Parsons turbines were actually under construction at St. Nazaire, intended for superheated steam. I may say that at present the question is only under examination. We hope to have the results this summer of a vessel building in Germany with Parsons turbines and a superheater of a different type, where the quantity of the gases passing through the superheater can be regulated by the chief engineer from his position in the engine-room, so that at last we shall have the records of the effect on the marine turbine, even though the superheater itself may or may not be always entirely successful.

Mr. Leslie S. Robertson (Member): Mr. Chairman and Gentlemen, in referring to the second paragraph of the paper, with regard to the difficulties occurring with hemp packing, which have already been alluded to by Professor Watkinson, I should like to add the lubrication difficulty. This is what has, I think, militated against the adoption of superheated steam, and certainly of highly superheated steam, and there was also the difficulty which they had in the early days of getting the material itself to stand the high superheat. With regard to the last paragraph on the first page of the paper, where the author says: "Since then the use of superheated steam with stationary engines has increased largely," this is so, but it may not perhaps be generally known in this room, where we are supposed to be talking about marine matters, that the use of highly superheated steam has also increased very largely for locomotive work, and there are, to my certain knowledge, over 2,700 locomotives running with highly superheated steam on one particular system (the Schmidt system). Several of the railway companies in this country are beginning to adopt this system, and, without mentioning names, I may say that a managing director of one of our leading railways mentioned to me the other day that he was getting something like 25 per cent. saving by superheating. Then, in regard to the use of valves, with highly superheated steam you cannot use flat slide valves, and the lubrication has to be of a positive nature. If you attempt to use a saturated steam design for superheated steam, you will inevitably get into trouble. You must approach the subject from the superheat side, and not from the saturated steam side. On page 173 I notice that the author has put up the pressure from 182 to 214 lbs. per square inch. I think it is preferable to keep the boiler pressure down, as you then get better results from your superheat, and you avoid all troubles in the boiler and elsewhere, inherent to the use of a higher pressure. This is a point in locomotive design especially, where it is not desirable to raise the pressure, but, on the contrary, to keep it down. In regard to the paragraph where the author speaks about the trials in Germany, Mr. Ridsdale has corrected a statement which might perhaps be read the wrong way. It is not a Pielock superheater that has been fitted on the *Ersatz Komet* at all, but a Schmidt superheater. The boat is somewhere between 15,000 to 20,000 H.P., and when we get the results from a boat of this size and importance they will be extremely interesting. I might, as I am on this question, mention that there are in Germany over 120 boats actually running to-day on the Schmidt system, so that it is not absolutely in its infancy; and though the author mentions France and England and America, I think he should have included our German friends, because they have done some very good work indeed in this direction.

I do not understand the expression on the top of page 176, where the author says: "which rises only a few degrees when additional power is required of the boilers, and falls again automatically directly

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the engines are stopped." I was much surprised to see the regularity of these curves, and I shall look forward with great interest to the time when Monsieur Godard, to whom I think we are greatly indebted for coming forward and giving us these particulars, has carried out the experiment suggested by Mr. Ridsdale. I think to many of us a superheater placed in the middle of the boiler where you cannot get at it or inspect it, especially when it has somewhere about 700 joints to keep tight, presents extreme difficulty. I admit that the difference of pressure is only a pound or two at the most, but one has an inherent dislike to placing anything inside a boiler where you cannot get at it and inspect it. I must confess that I prefer a system with the tubes outside and accessible as used by Mr. Schmidt, and where you are able to regulate, as they can do, the actual quantity of gases passing through the superheater. To get a good efficiency out of a superheater you must have a steady temperature and complete control over the gases, and you must be able to control them not only automatically, but also by hand. With regard to the figures given for consumption, I do not think that these high figures are going to be obtained as a general rule, although they are very interesting. I had some figures passing through my hands the other day of some steam trawlers which had been fitted with superheaters on the Schmidt system. In two boats exactly similar, except for the superheaters, the net results for the year's working were 3½ tons of coal per diem for the superheated boat and 4½ tons for the saturated steam boat—or a saving of over 25 per cent. Still, these are high figures, and one feels inclined to accept them with reserve till they are confirmed by further extended trials.

The CHAIRMAN (the Hon. C. A. Parsons, C.B., M.A., F.R.S., D.Sc., Member of Council): The discussion on this interesting paper will be sent to Monsieur Godard for his reply, and we should, I am sure, tender him our thanks for his most interesting paper on this important subject.

WRITTEN REPLY TO THE DISCUSSION.

Monsieur F. GODARD (Member): Mr. Chairman and Gentlemen, nothing has been observed with regard to the influence of superheaters upon the working of the condenser except that the vacuum is much improved, as may be seen by the diagrams published in "Engineering" of January 18, 1907. In these it is shown that notwithstanding her higher powered engines, the *Rance* (with superheaters) obtained a better vacuum than the *Garonne* (without superheaters), the engines on the two ships being otherwise identical. This may be rationally explained by the fact that the weight of steam passing into the condenser is less where the consumption of steam is smaller. In fact, the vacuum depends much more upon the absence of leakage than upon the weight of steam passing into the condenser.

Mr. Rosenthal appears surprised to find the Pielock superheaters working without difficulty for over a year, but I would mention that two locomotives on the St. Gothard railway, fitted with these superheaters, have been in constant service for over four years without giving trouble, and this on a very heavy line, where steam is frequently shut off directly after raising the fires to their extreme limit and with a temperature in the tubes of the superheaters far higher than in marine boilers. Moreover, these superheaters are of an early design, when their construction was nothing like as accurate as it is at present.

Replying to Professor Watkinson, I share his opinion that engines fitted with Corliss valves can work with a moderate amount of superheat, but this system involves allowing a good deal of play in the working parts, in order to avoid friction and obtain satisfactory results. It is difficult, therefore, to obtain tight joints and a low coal consumption, and drop valves are found to be more satisfactory, as permanently tight joints may be obtained without undue friction. Moreover, drop valves require less power to work them than slide valves. With the piston rings we have never had any trouble, and have used those of the Ramsbottom type, constructed to a special design of our own. I have often heard of the lubrication troubles referred to by Professor Watkinson in cases where the oil was of bad quality,

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but we only use a mineral oil suitable for the purpose, and this has given us no trouble. With regard to consumption, I did not state that the coal consumption could be reduced by 30 to 35 per cent., but I was referring to the steam consumption, as pointed out by the Chairman. There is always a considerable difference between economy of steam and of coal, because with the former no account is taken of the temperature of the steam or the efficiency of the boiler. It may be said that upwards of 15 to 25 per cent. reduction in coal consumption can be obtained by the use of superheaters.

With regard to the durability of the superheater tubes, it may be interesting to state the rule that we follow in determining the position of the Pielock superheaters in the boilers. The distance from the superheater to the boiler is arranged so that the gases entering the superheater cannot attain a temperature that would cause the tubes to become red hot, and even with a roaring furnace and a sudden stop, the tubes, although surrounded with stagnant gases, will remain well below a dull red heat. It is to be noted that in all boiler installations the heat of the furnace drops rapidly when the demand for steam is reduced, otherwise the boiler pressure would be excessive. It is due to this fact that in the Pielock superheater the temperature is self-regulating.

Then again, leaky tubes need not be feared, as the difference of pressure inside and outside the superheaters is extremely small, as stated by Mr. Ridsdale. To that gentleman's remarks I would only add that leakage cannot occur at the junction of the tubes with the tube-plates if the work has been properly carried out; the apparatus, if carefully constructed, can be safely left inside the boiler without requiring attention. Mr. Ridsdale appears afraid of excessive temperature if the engines are started after a sudden stop when at full speed. Now, in the first place, when a stop occurs under these conditions, the fans are immediately stopped, and the fires drop rapidly, consequently the temperature of the tubes does not rise unduly; and further, the boilers will be replenished during this time, so that if the engines are started again somewhat suddenly, there is more danger of a sudden inrush of water than of steam, and it is only on account of this risk that we have not carried out the experiments suggested by Mr. Ridsdale. If it is, nevertheless, desired to regulate the temperature of the superheated steam, as delivered to the engines, by means of a mixture of saturated steam, it is no more difficult to do so with the superheater described in the paper than with any other. Such an arrangement entails undesirable complications, however, and the same result can be obtained much more simply by spraying water into the superheater tubes, and the action of the sprayed water is far more effectual than that of saturated steam.

From Mr. Ridsdale's concluding remarks, he appears to have some doubts as to the successful working of marine turbines with superheated steam. It is, however, certain that such a development is a most desirable one, and it would be interesting to know the opinion of Messrs. Brown, Boveri & Co. on the subject, as they have had considerable experience in the use of superheated steam with land turbines. Mr. Ridsdale mentions the satisfactory working of the Lentz valves on the *Pérou* and *Rance*, but he also quotes the case of a reciprocating engine working at about 300 revolutions, in which some difficulties have been met with. In this case the troubles came, not from the working of the engines, which was satisfactory, but from the very restricted space at the disposal of the designers. It may be stated with confidence that Lentz valves work equally well in cargo boats with engines running at 75 to 90 revolutions, as in high-speed engines for either land or marine work, and their field of usefulness is, therefore, very extensive, especially in locomotive engines.

Replying to Mr. Leslie Robertson, I am glad that he mentioned the employment of superheated steam in locomotives; I omitted any reference thereto in my paper, as it was devoted to the marine aspect of the question. I am quite aware that the Schmidt superheater is largely used in Germany, and that its use in locomotives throughout the world is increasing. The Pielock superheater, on the other hand, was more recently designed, and has, therefore, been less time upon the market, and has consequently not been so extensively employed; but it may be seen in use or on trial in many countries, where its simplicity and easy working have been recognised. As already stated, two locomotives fitted with this superheater have been running for over four years on the St. Gothard railway, thus disposing of the question of durability. As to its not being controlled by hand, so far from this being a drawback, it is,

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on the contrary, a great advantage, as there is thus no personal error to fear, and the engine room staff are not burdened with additional duties. As regards the tightness of the joints, it may be remarked that in the Pielock superheater the joints are only under a very slight pressure, whereas in the Schmidt system there is the whole boiler pressure, and the temperature of the superheated steam besides. We find no difficulty either with piston rings or rods, and the use of the valves described in the paper does away with all troubles inherent to slide or piston valves, so that one can use steam at the pressure and temperature most convenient for each particular case. Mr. Robertson advocates lowering the pressure, but I see no good reason for such a measure, except the desire to avoid troubles which have been met with in the type of superheater which he favours. In conclusion, I wish to thank the President and Members of the Institution for the interest which has been shown in my paper, and I hope at some future date that additional information may be forthcoming on the use of superheated steam on board ship, and more especially in turbine vessels.

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RESULTS OF FURTHER MODEL SCREW PROPELLER EXPERIMENTS.

By R. E. FROUDE, Esq., F.R.S., LL.D., Honorary Vice-President.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 10, 1908; the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D., President, in the Chair.]

I.—GENERAL SCOPE OF EXPERIMENTS.

§1. THE experiments on model screw propellers, with which this paper is concerned, are an extension of those made at the Torquay Tank in 1884, and dealt with in the paper which I read before this Institution in 1886.* And since the present experiments, while covering a good deal of fresh ground, covered also the whole ground of the previous ones—so far, at least, as is relevant to present-day conditions—and were carried out, in some ways, on improved methods, their results may be said to supersede those of the earlier experiments.

§ 2. The principal points in which the present experiments exceeded in scope the previous ones, were as follows.

(i.) Lower values of pitch ratio included. [The highest values included in the old experiments were at the same time discarded; but these were quite outside the range of present practice.] This downward extension of the pitch-ratio range in the new experiments, brings in pitch ratio as an important factor affecting efficiency, since it is in the region below the range of the old experiments that pitch ratio influences efficiency in an important way.

(ii.) Three-bladed as well as four-bladed propellers were tried [in the old experiments the model propellers were all four-bladed].

(iii.) Variation of width proportion of blade was tried, ranging from that of the old experiments up to about twice the width.

(iv.) Difference in blade shape; by including the wide-tip pattern, in addition to the elliptical pattern solely used in the old experiments.

The blade outlines, sections, &c., are shown in Figs. 1 to 4 (Plate XVIII.). For other particulars of the series of model propellers tried, see Appendix I.

II.—METHOD AND GENERAL CONDITIONS OF EXPERIMENT.

§ 3. *Method.*—The experiments were the usual kind of what are described as screw experiments "in open," namely, in undisturbed water (without model in front), the

* Trans. I.N.A., Vol. XXVII. page 250.

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screw being mounted on the forward end of the shaft which drives it, the whole advancing through the water at a prescribed speed, the screw driven at prescribed revolutions per minute, and the thrust and turning moment being measured. Each set of experiments on each screw consisted of some 20 runs at the same speed, at different slip ratios; including a run or two, at beginning and end, with screw off, for eliminating constant resistances of apparatus. This series of runs, which occupied some 2½ hours in the middle of each day, was daily both preceded and followed by a series of experiments, with truck stationary and a turbine brake dynamometer substituted for the screw, to measure the "working" friction of the driving gear.

§ 4. *Diameter of Screws.*—Uniformly 0·8 ft. [as compared with 0·68 ft. in the old experiments].

§ 5. *Immersion* (to centre of shaft).—Uniformly 0·64 ft. [viz., 0·8 of the diameter, as in the old experiments].

§ 6. *Speed* (of advance).—300 ft. per minute [as compared with 206 ft. per minute in the old experiments]. This was the highest speed at which it would have been possible to obtain the desired range of slip ratio, without overstraining the driving gear.

§ 7. *Boss of Screws.*—The boss common to all the model screws (see Fig. 4) was (as usual in our model screws) of the smallest practicable character. Some special experiments made with ordinary large bosses showed the effect of a large boss to be material, partly in its resistance, and still more in an increase of revolutions per minute, due presumably to stream line action; but, as both these effects are intimately involved with those of the bearings or shaft tubes, which in the ship immediately precede the propeller, it seems best, where all these features are absent, as in the experiments with which we are here concerned, to suppress the boss so far as possible.

§ 8. *Skew-back of Blades.*—The model screws employed in the regular series of experiments were without skew-back; but experiments were made with four additional model screws having a skew-back of 15° on the driving face, otherwise similar to four of the regular series,[1] and the skew-back was found to make no material difference in result.

III. Analysis and Reduction.

§ 9. *Basis.*—It is perhaps in the system of analysis and reduction of the results, more than anything else, that the present series of experiments differs from the previous. The introduction of width proportion and shape of blades as varying conditions, and the recognition of pitch ratio as having an important influence on efficiency, in any case necessitated some change; but also, the possibility had presented itself to me, in course of study of the results of the previous experiments, of so harmonising the results that the relation of thrust to revolutions per minute could

[1] See Appendix I. These four skew-back screws were generally similar to Nos. 7, 8, 9, and 31 of the list.

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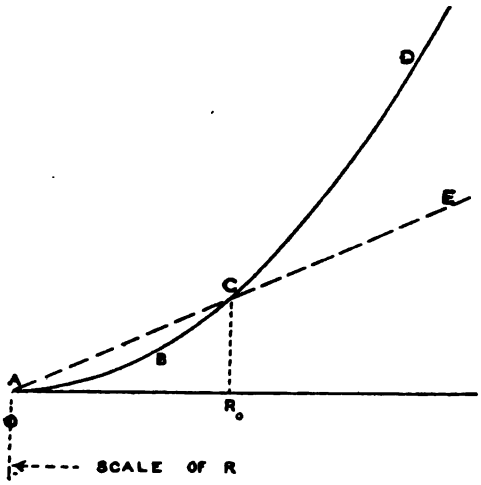
be calculated for a screw of any diameter and pitch by means of a simple formula, without using any diagrams or tables at all.

§ 10. And apart from the convenience of being able to embody the results in such a form, it is, of course, always desirable, when possible, in harmonising the minor and accidental irregularities of a system of experiment results, to use a mathematical expression which has some at least approximate theoretical relevance, in place of a mere graphic fairing of spots by a curve. The analysis, then, of the series of thrust and efficiency curves yielded by the experiments on the individual model screws, was in the first instance based on the following simple formula for thrust in terms of revolutions per minute :—

$$T = a R^2 - b R \qquad (1)$$

Where T = thrust; R = revolutions per minute; a = a co-efficient depending on dimensions, &c., of screw; b, one depending on the speed and pitch.

§ 11. This formula embodies the following idea, which under certain ideal conditions would be theoretically correct.[2] In any screw, revolving in still water at various rotary speeds without axial advance, the thrust will, of course, be proportional to the square of the rotary speed. This fact is expressed by the term $a R^2$ of the formula, represented by the ordinates of the parabola A B C D in the marginal diagram. If now we suppose the screw, while still revolving as before at various rotary speeds, to have a definite forward axial linear speed of advance, V, as well, there will, of course, then be a certain definite rotary speed (revolutions per min. = R_0, say), at which the thrust will be zero, below which it will be negative, and above which it will be positive, but of decreased amount. And the formula expresses this decrease of thrust in terms of revolutions per minute, by the negative term $b R$, represented by the ordinates to the straight line A C E, cutting the parabola at C, namely at R_0, the revolutions of zero thrust. Thus the straight line A C E becomes in effect the zero line for the curve C D, regarded as the thrust curve for the speed of advance, V; and, similarly, the same parabola A B C D may be made to furnish the thrust curve for

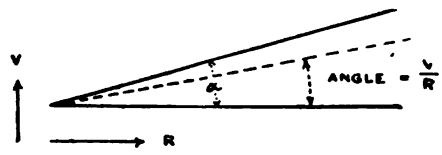


[2] Imagine an elementary blade mounted on the end of a resistless spoke, revolving at circumferential speed R, and standing at an angle α from the plane of rotation; the whole having an axial speed V relatively to the fluid. Then (see Fig. below), assuming α "small," if R_0 be the circumferential speed for zero thrust, we may put—

$$\alpha = \frac{V}{R_0},$$

and—

$$\text{Thrust} = T = (\text{say})\, k R^2 \left(\alpha - \frac{V}{R}\right) = \left(k \frac{V}{R_0}\right) R^2 - (k V) R.$$



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the same screw at any speed, by drawing accordingly the sloping line A C E which represents the term $(-b\,R)$ of the formula.

§ 12. Even before the experiments herein described were begun, I had found that this formula expressed the thrust curves of experiment, in general, remarkably well; since, therefore, a new reduction was in any case necessary, this formula was unquestionably the right basis for at any rate the primary analysis of the results.

§ 13. *Thrust.*—Continuing the study of the formula, it will be seen that, if, as is most convenient for such analysis, and as has been done in this case, we take as a conventional measure of the pitch the travel per revolution at the revolutions per minute R_0 (of zero thrust), so that

$$P = \frac{V}{R_0},$$

and, again, observing that for zero thrust at revolutions $= R_0$ we must have $a R_0^2 = b R_0$, we can eliminate the co-efficient b in terms of a, V, and P. And, assuming the co-efficient a to have been correctly obtained for a screw of specific design and unit diameter (diameter $= D = 1$), we obtain from equation (1) above, the following two alternative equations for thrust[3]:—

$$T = \frac{a}{p^2} D^2 V^2 \frac{S}{(1-S)^2} \qquad (2)$$

$$T = a D^4 R^2 S, \qquad (3)$$

where—

$$p = \frac{P}{D},$$

or pitch-ratio, and S = slip-ratio as ordinarily reckoned, viz. $(R - R_0) \div R$. The former of these, expressing thrust in terms of speed and slip ratio, is perhaps the most intelligible; while the latter is often more convenient for computation.

§ 14. To enable the thrust for given speed and revolutions, to be calculated by either of these formulæ for a propeller of any given design and dimensions, it only needed to determine the co-efficient a as affected by difference of design, the principal elements in which may be taken to consist in (i.) pitch ratio; (ii.) type, and blade width proportion.

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[3] Substituting, in equation (1) of § 10, $a R_0$ for b, we get—

$$T = a R^2 - a R_0 R = a R^2 \left(1 - \frac{R_0}{R}\right) = a R^2 S.$$

This is for unit diameter. For propellers of uniform design, area increases as (diameter)^2; and, for given revolutions, so does thrust per unit of area as affected by speed; thus giving equation (3) of text. Next, remembering that—

$$(1 - S) = \frac{V}{R P},$$

and substituting for R in equation (3), we get—

$$T = a D^2 V^2 \frac{D^2 S}{P^2 (1-S)^2} = \frac{a}{p^2} D^2 V^2 \frac{S}{(1-S)^2}.$$

N.B.—In the analysis of the model propeller results, V has been taken in hundreds of feet per minute, and R in hundreds of revolutions per minute.

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§ 15. It was found that the effects of these two principal elements might be taken as independent of one another, and that, as regards (i.), a might be most correctly taken as proportional to $p\,(p + 21)$. As regards (ii.), the value $\frac{a}{p(p+21)}$, which, as just seen, is constant for varying pitch-ratio, was taken as the expression for the "blade-factor" B; the purpose of which is to denote what may be called the thrust capacity of the propeller, as dependent on type, *i.e.*, whether 3 blade elliptical, 3 blade wide-tip, or 4 blade elliptical; and within each of these types, on width proportion of blade. The value of this blade factor, B, as obtained from the experiments, and as dependent on these variants, has been indicated by the ordinates of three curves[4] (see Fig. 5, Plate XVIII.) respectively proper to the three types just mentioned, on an abscissæ scale representing blade width proportion, as indicated by "disc area ratio," namely, ratio of total blade area to disc area. (See Appendix I, last three lines.)

§ 16. At the same time, for a final test of the formula of equation (1), as accurately expressing the variation of thrust with revolutions, the thrust values of all the individual propellers, at a series of slip-ratio values, were carefully compared with thrust values calculated by formula; and on this information the thrust formula was corrected by multiplying the right-hand side by $1{\cdot}02\,(1 - {\cdot}08\,S)$. Making this correction, and also substituting for a its value in terms of the blade factor B, just referred to, equation (2) of § 13 becomes the final thrust formula, as follows:—

$$T = D^2 V^2 \times B \frac{p + 21}{p} \times \frac{1{\cdot}02\,S\,(1 - {\cdot}08\,S)}{(1 - S)^2}. \qquad (4)$$

§ 17. To facilitate calculations, a curve was computed expressing the last factor (involving S only) as an ordinate ($= y$), to a base ($= x$) expressing revolutions and pitch relatively to speed, as indicative of the slip ratio S. This curve, commonly called the "$x\,y$" curve, appears in Fig. 6. Conveniently for ship screw calculations, the numerical co-efficients used in the computation of this curve were chosen for expressing, not thrust, but "thrust horse-power" (or T.H.P.) = H; speed = V, in knots; Revolutions = R, in hundreds; diameter = D, in feet. We thus get as the expressions for x and y as follows[5]:—

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[4] See also the calculated "B" ordinates in Appendix II. The values of B as originally obtained from the model experiments in fresh water have, in these curves and tables, been corrected for salt water, conveniently for use in ship calculations.

[5] In regard to the expression for x,

$$1{\cdot}0133 = \frac{6{\cdot}08}{6} = \frac{\text{speed in hundreds of feet per minute}}{\text{speed in knots.}}.$$

In regard to that for y; from equation (4) [§ 16].

$$\frac{T}{D^2 V^2}\left[= \frac{T\,V}{D^2 V^3}\right] = B\frac{p + 21}{p} \cdot \frac{1{\cdot}02\,S\,(1 - {\cdot}08\,S)}{(1 - S)^2}.$$

Then, converting V from 100's feet per minute [see Footnote [3] to § 13] into knots and T V into H, this equation becomes—

$$\frac{H\,\frac{33000}{100}}{D^2 V^3 \left(\frac{101{\cdot}33}{100}\right)^3} \cdot \frac{p}{B\,(p + 21)} = \frac{1{\cdot}02\,S\,(1 - {\cdot}08\,S)}{(1 - S)^2},$$

or—

$$\frac{H}{D^2 V^3} \cdot \frac{p}{B\,(p + 21)} = {\cdot}0032162\,\frac{S\,(1 - {\cdot}08\,S)}{(1 - S)^2}.$$

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$$x = \frac{R\,p\,D}{V}\left[=\frac{1{\cdot}0133}{1-S}\right]. \qquad (5)$$

$$y = \frac{p}{B(p+21)} \times \frac{H}{D^2 V^2}\left[= {\cdot}0032162\,\frac{S\,(1-{\cdot}08\,S)}{(1-S)^2}\right]. \qquad (6)$$

§ 18. *Efficiency.*—We have next to consider the efficiency reduction. The variation of efficiency with pitch ratio makes it impossible to bring all the results under one single efficiency curve as was done for the 1884 experiments (1886 Paper). It was therefore necessary to obtain a series of efficiency curves for a convenient series of round-number pitch ratios,[6] such as appear in Fig. 6 (Plate XIX.), plotted to the base "x" of the "$x\,y$" curve. These are correct for the 3 blade elliptical type of screws, and for a standard disc area ratio of 0·45. To correct them for another type or disc area ratio, or both, a uniform efficiency deduction must be made, of ·020 for 3 blade wide-tip, or ·0125 for 4 blade elliptical, also one appropriate to the disc-area ratio and pitch ratio in question, as indicated by the ordinates of the efficiency correction curves, to base disc area ratio, in Fig. 5 (Plate XVIII.).

§ 19. Efficiency for a pitch ratio intermediate between those for which the series of efficiency curves are shown, has to be obtained by interpolation, but in nearly all cases of practical occurrence the successive curves are so near together that this can readily be done by eye.

§ 20. The series of efficiency curves for the series of round-number pitch ratios, already referred to, were obtained from those for the four pitch ratios of actual experiment, which appear in Fig. 8 (Plate XX.), by means of cross curves, such as are shown in Fig. 9. In the former diagram the curves are carried to unity slip ratio, and in the latter to zero of pitch ratio; in both cases, of course, far beyond the experimental data; this being done to help guide the directions of the curves at the confines of the data. In thus extending the compass of the curves, as well as in fairing them throughout, regard was had, so far as the experiment data left room for any question, to the theoretically calculated curves in Figs. 10 and 11, on which I comment in Appendix III.

§ 21. *Design.*—The "$x\,y$" curve, with its companion efficiency curves, serves perfectly for analysis of steam trials, where it is only needed to calculate thrust horse-power and efficiency, for known revolutions per minute and speed, with known propeller dimensions. But, for design, where it is generally desired to calculate propeller dimensions suitable to given horse-power, revolutions, and speed, a difficulty arises from the fact that diameter, an unknown quantity, enters into both x and y.[7]

[6] This diagram, as well as that in Fig. 7 (Plate XIX.), to avoid confusion, shows curves for an abridged series of pitch ratios. Calculated ordinates for all the curves of importance, for the entire series of pitch ratios, are given in Appendix II.

[7] The $x\,y$ curves *can* be used for design, though with indifferent convenience, as follows: If we choose a value for p, a series of chosen values of x, with the corresponding ones of y, determine corresponding ones of D from V and R by equation (5), and thence ones of B from V and H by equation (6). Thus we can get curves of D, to base B (or propeller area if preferred), for a series of values of p. The main objection is that for chosen p there is a very small range of choice of x without exceeding the practical range of B.

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So, for purposes of design, there were devised the curves in Fig. 7,[8] for which the abscissa, "C_A," is determined solely by the known horse-power, revolutions, and speed; while the corresponding ordinate, "C_0," determines the unknown diameter in terms of the same known quantities. The expressions for these two values are—

$$C_A = \frac{R^2 H}{B V^5} \left(= \frac{p + 21}{p^5} \cdot x^2 y \right) \qquad (7)$$

$$C_0 = \frac{H}{B D^2 V^3} \left(= \frac{p + 21}{p} \cdot y \right) \qquad (8)$$

Unlike the "$x\,y$" curve, this curve differs with pitch ratio; a series of curves is therefore shown, for the same series of pitch ratios as the series of efficiency curves accompanying the "$x\,y$" curve, which series of efficiency curves is also reproduced on the C_A base to accompany the curves of C_0.

§ 22. Unfortunately, it is never practicable to treat pitch ratio as an unknown quantity. Our usual practice in using these data is to obtain diameter and efficiency for two or more of the pitch-ratio values for which curves are given, each for two or more values of disc-area ratio, and plot the results on a base of total blade area.[9] In this way, the diameter and efficiency for any intermediate pitch ratio is indicated.

§ 23. *Nominal Pitch.*—It may be recollected that in the system of analysis and reduction just described, the pitch P, corresponding both to the pitch ratio p, and the slip ratio S, is throughout taken as equal to the travel per revolution for zero thrust; in other words the revolutions of zero thrust are taken as the zero of the slip-ratio scale. Comparison of the figures which we have calculated from these data with the realisations in actual ships, have led to the conclusion that the pitch or pitch-ratio figures used or obtained in these calculations, which we may term "analysis pitch," should be taken as 1·02 times the nominal (or driving-face) pitch for ship.[10]

---

[8] See Note 6 to § 18.

[9] The disc-area ratio being reckoned [see Appendix I.] for total area of outline without boss allowance, in computing the total blade area for ship propeller from the diameter and disc-area ratio, a discount must be made, to allow for portion of area covered by boss. In our general practice at Haslar this discount is 20 per cent.

[10] For present purposes I prefer this result of net experience to any estimate based on the relations between "analysis" pitch and constructional pitch in the model screws, partly because I question the adequate nicety of the pitch moulding of the model propellers, for such a purpose, partly because of the differences of condition between ship propellers in use, and model propellers under experiment, some of which are referred to above in § 7.

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APPENDIX I.

Particulars of Regular Series of Model Propellers.

Type.	Three Bladed, Elliptical.											
Reference No.	1	2	3	4	5	6	7	8	9	10	11	12
Blade-width Ratio	·4	·55	·7	·4	·55	·7	·4	·55	·7	·4	·55	·7
Disc Area Ratio ..	·3	·413	·525	·3	·413	·525	·3	·413	·525	·3	·413	525
Pitch Ratio[11]	·885			1·09			1·315			1·53		
Type.	**Three Bladed, Wide Tip.**											
Reference No.....	13	14	15	16	17	18	19	20	21	22	23	24
Blade-width Ratio	·6	·8	1·0	·6	·8	1·0	·6	·8	1·0	·6	·8	1·0
Disc Area Ratio ..	·45	·6	·75	·45	·6	·75	·45	·6	·75	·45	·6	·75
Pitch Ratio[11]	·885			1·09			1·315			1·53		
Type.	**Four Bladed, Elliptical.**											
Reference No. ..	25	26	27	28	29	30	31	32	33	34	35	36
Blade-width Ratio Disc Area Ratio ..	·4	·55	·7	·4	·55	·7	·4	·55	·7	·4	·55	·7
Pitch Ratio[11]	·885			1·09			1·315			1·53		

See also text, §§ 3—8. For outlines and sections of blades, and shape of boss, &c., see Figs. 1 to 4 (Plate XVIII.). For the elliptical propellers, the developed outlines are ellipses of major axis equal to propeller radius, and the blade-width ratio is the ratio of minor to major axis. The developed outlines of the wide-tip propellers are formed from elliptical outlines for the same reputed blade-width ratio by making, for any radius, $= r$,

$$\frac{\text{Wide-tip width ordinate}}{\text{Elliptical width ordinate}} = \left(\frac{1}{2} + \frac{r}{R}\right)$$

where R = propeller radius. Hence, for the same blade-width ratio, the wide-tip and elliptical outlines have the same total area. For the purpose of the analysis, and for relating disc area ratio to blade width ratio (see text, § 15), the entire area of the elliptical or wide-tip outline thus obtained is reckoned, without any allowance for portion covered by boss.

[11] *I.e.*, "Analysis" Pitch (see § 23). The figures given are the approximate means of the (slightly various) figures obtained by analysis for the different screws constructed for a common nominal or driving-face pitch.

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APPENDIX II.

TABULATED ORDINATES FOR CURVES OF x, y, C_0, C_A, B, AND EFFICIENCY.
[See §§ 15, 17, 18, 21, and Figs. 5, 6, 7 (Plates XVIII. and XIX.).]

Slip Ratio.	x	y	Pitch Ratio ·8.			Pitch Ratio ·9.		
			C_A	C_0	Effcy.	C_A	C_0	Effcy.
0	1·013	0	0	0	0	0	0	0
·02	1·034	·000067	·003044	·001822	·190	·002148	·001627	·209
·04	1·056	·000139	·006598	·003791	·312	·004656	·003385	·341
·06	1·078	·000217	·010752	·005922	·406	·007586	·005287	·439
·08	1·101	·000302	·015595	·008230	·479	·011000	·007347	·511
·10	1·126	·000394	·021262	·010732	·531	·01500	·009583	·562
·12	1·152	·000494	·02789	·013451	·571	·019664	·012012	·600
·14	1·178	·000602	·035575	·016400	·601	·02510	·014644	·629
·16	1·206	·000720	·04459	·019618	·622	·031461	·017516	·650
·18	1·236	·000849	·05519	·02312	·639	·038948	·020648	·666
·20	1·267	·000989	·067528	·02695	·650	·047638	·024061	·675
·22	1·299	·001142	·082065	·031125	·655	·05790	·02779	·680
·24	1·333	·001311	·09916	·03571	·657	·06995	·031885	·680
·26	1·370	·001495	·11933	·040735	·655	·08420	·036370	·678
·28	1·407	·001698	·14314	·04627	·651	·10096	·04131	·673
·30	1·448	·001922	·17150	·05236	·647	·12098	·04675	·666
·32	1·491	·002169	·20510	·059092	·642	·14470	·05276	·659
·34	1·535	·002442	·24500	·06653	·635	·17284	·05940	·650
·36	1·583	·002745	·29296	·07480	·627	·20666	·06678	·640
·38	1·635	·003086	·35120	·08408	·618	·24772	·07508	·630
·40	1·689	·003457	·41986	·09420	·608	·29621	·084105	·618
.42	1·747	·003880	·50418	·10571	·596	·35570	·09439	·604
·44	1·810	·004354	·60700	·11861	·584	·42830	·10591	·590
·46	1·877	·004887	·73280	·13314	·570	·51710	·11890	·574
·48	1·949	·00549	·88750	·14959	·556	·62630	·13357	·558
·50	2·027	·006175	1·07960	·16823	·542	·76160	·15022	·543

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Slip Ratio.	Pitch Ratio 1·0.			Pitch Ratio 1·1.			Pitch Ratio 1·2.		
	C_A	C_0	Effcy.	C_A	C_0	Effcy.	C_A	C_0	Effcy.
0	0	0	0	0	0	0	0	0	0
·02	·001573	·001471	·228	·001187	·001343	·241	·000919	·001237	·255
·04	·003410	·003061	·370	·002573	·002795	·394	·001991	·002574	·425
·06	·005557	·004781	·470	·004192	·004365	·493	·003244	·004020	·521
·08	·008058	·006644	·540	·006081	·006067	·563	·004705	·005587	·585
·10	·010986	·008666	·590	·008290	·007913	·612	·006416	·007288	·631
·12	·01440	·010860	·625	·010870	·009918	·648	·008408	·009133	·664
·14	·018378	·013240	·652	·013871	·012091	·671	·010736	·011136	·687
·16	·023045	·01584	·671	·01739	·014464	·690	·013455	·01332	·706
·18	·028522	·018670	·685	·021525	·017048	·703	·016658	·01570	·716
·20	·03489	·021758	·695	·026320	·01987	·711	·020378	·01830	·723
·22	·042405	·025130	·700	·03200	·022947	·715	·02476	·02113	·726
·24	·051235	·028831	·701	·038666	·026326	·715	·02992	·024245	·726
·26	·06186	·03299	·699	·046545	·030035	·711	·03602	·02766	·721
·28	·073965	·037358	·693	·055815	·034113	·705	·043195	·031415	·715
·30	·08861	·042275	·685	·06686	·03860	·696	·05175	·035555	·705
·32	·10600	·047708	·675	·07998	·043565	·685	·06189	·04012	·694
·34	·12657	·053715	·663	·09552	·049055	·672	·07393	·04517	·681
·36	·15138	·060391	·651	·11425	·05515	·659	·08822	·05078	·665
·38	·18150	·06790	·638	·13693	·06200	643	·10596	·05709	·648
·40	·21700	·07606	·624	·16372	·06945	·628	·12670	·06396	·631
·42	·26050	·08536	·610	·19660	·07795	·610	·15210	·07178	·611
·44	·31370	·09578	·593	·23678	·08747	·591	·18320	·08054	·591
·46	·37865	·10750	·576	·28580	·09818	·573	·22115	·09041	·571
·48	·45870	·12078	·558	·34620	·11030	·554	·26788	·10157	·551
·50	·55800	·13584	·540	·42113	·12405	·535	·32585	·11425	·530

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Slip Ratio.	Pitch Ratio 1·3			Pitch Ratio 1·4			Pitch Ratio 1·5		
	C_A	C_o	Effcy.	C_A	C_o	Effcy.	C_A	C_o	Effcy.
0	0	0	0	0	0	0	0	0	0
·02	·000726	·001147	·274	·000584	·001070	·285	·000475	·001003	·305
·04	·001573	·002386	·446	·001265	·002226	·465	·001033	·002087	·480
·06	·002563	·003727	·540	·002062	·003477	·558	·001684	·003260	·570
·08	·003718	·005180	·601	·002990	·004832	·616	·002442	·00453	·626
·10	·005068	·006756	·645	·004077	·006303	·656	·003329	·005908	·665
·12	·006643	·008468	·677	·005345	·007900	·686	·004364	·007406	·695
·14	·008479	·010323	·699	·006822	·009631	·707	·005571	·009029	·715
·16	·01063	·012350	·715	·00855	·011520	·721	·006984	·01080	·728
·18	·013159	·014555	·725	·010584	·013579	·732	·008642	·012728	·737
·20	·016091	·01696	·731	·012946	·015825	·738	·010570	·014832	·743
·22	·019571	·019590	·733	·015735	·018274	·740	·012848	·017131	·743
·24	·023635	·02248	·731	·019010	·02097	·737	·015522	·019655	·741
·26	·028448	·02564	·727	·022830	·02392	·731	·018685	·022422	·735
·28	·03412	·02912	·720	·027445	·02717	·722	·022414	·02547	·726
·30	·04088	·03296	·710	·032880	·030745	·712	·02684	·02882	·715
·32	·04890	·037195	·697	·03933	·03470	·701	·03212	·032524	·701
·34	·05839	·041875	·683	·046917	·03907	·686	·03835	·03662	·686
·36	·06982	·04708	·668	·05617	·04392	.669	·04587	·04117	·668
·38	·08371	·05293	·650	·06734	·04938	·650	·05497	·04628	·650
·40	·10008	·059295	·630	·08050	·05531	·631	·06574	·05185	·631
·42	·12015	·06654	·610	·09667	·062085	·609	·07894	·058195	·609
·44	·14470	·07467	·590	·11640	·06966	·588	·09506	·06530	·585
·46	·17465	·083805	·569	·14051	·07820	·566	·11475	·07330	·562
·48	·21155	·09416	·547	·17020	·08785	·544	·13900	·08235	·540
·50	·25735	·10590	·526	·20704	·09880	·521	·16910	·09262	·516

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B VALUES. [See § 15.]

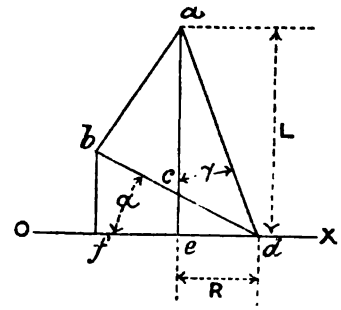
Disc Area Ratio.. ..	·30	·35	·40	·45	·50	·55	·60	·65	·70	·75	·80
3 Blades, Elliptical ..	·0978	·1020	·1050	·1070	·1085	·1100	·1112	·1124	·1135	·1147	·1157
3 Blades, Wide Tip ..	·1045	·1097	·1126	·1148	·1166	·1182	·1195	·1207	·1218	·1230	·1242
4 Blades, Elliptical ..	·1040	·1106	·1159	·1197	·1227	·1249	·1268	·1282	·1294	·1306	·1318

APPENDIX III.

THEORETICAL EFFICIENCY CURVES.

The curves referred to in § 20 and shown in Figs. 10 and 11 (Plate XX.), have been computed on three different bases. No. 1 of these is the solution of Mr. William Froude's paper of 1878 [Trans. I.N.A., Vol. XIX. p. 47], obtained from the rotary and thrust components of the estimated edgewise and normal resistance of an elementary plane, mounted on a revolving radius, obliquely to the plane of rotation. The other two are the same in principle (slightly different mathematics), but the resistance forces of the supposed plane or blade are obtained from the experiments made at Haslar some few years ago on planes and blades moving obliquely at various angles.[12] In No. 2 the plane was oval and thin, with edges symmetrically

[12] The planes or blades were tried at various angles of obliquity from the path, and the components of force, R in line of path, and L transverse to it, measured. I am indebted to Mr. Arnulph Mallock for the following charmingly simple general solution for the efficiency of such plane or blade regarded as a *reaction* instrument of any kind, *e.g.*, a leeboard, hydroplane, rudder, or blade of a propeller. In the figure in margin, if O X be the path of the plane (O towards X), let $d\,e$, $e\,a$, represent in magnitude and direction the forces R, L, respectively; and $d\,a$ their resultant; the angle of which in rear of the normal to the path ($= e\,a\,d$), is denoted by γ. To satisfy the triangle of forces, if the plane is kept in motion by a driving force, acting at an angle α from the line of motion, and represented by, say, $b\,d$, it must deliver a useful reaction represented by $b\,a$; and of these two forces the components in line of motion are $f\,d$, $f\,e$, respectively. The speed for both these components being the same, viz., that of the plane, and the corresponding rectangular components involving no movement, and therefore no energy, we can express the efficiency thus :—



$$\text{Efficiency}\left[=\frac{\text{work realised by useful reaction } b\,a}{\text{work expended by driving force } b\,d}\right]=\frac{f\,e}{f\,d} \qquad (9)$$

For the particular case of such plane or blade mounted on a revolving radius and treated as an elementary screw propeller, as in Mr. W. Froude's solution, the directions of $b\,d$, $b\,a$, must be taken as lying in plane of rotation and of axis respectively, the angle $a\,b\,d$ then being a right angle, and the angle $e\,a\,b$ therefore equal to $b\,d\,f = \alpha$. Then since $\frac{b\,c}{b\,d}=\frac{f\,e}{f\,d}$, we get—

$$\text{Efficiency}\left[=\frac{b\,c}{b\,d}\right]=\frac{\tan\alpha}{\tan(\alpha+\gamma)} \qquad (10)$$

which was the expression used in computing the curves for bases Nos. 2 and 3.

It is worthy of note that, by differentiation (or equally by a simple geometrical construction from the figure), we get for the condition of maximum efficiency—

$$\tan 2\alpha = \operatorname{cotan}\gamma\left[=\frac{L}{R}\right] \qquad (11)$$

Also, that, on the assumptions of Mr. W. Froude's 1878 paper, $\theta+\frac{k}{\theta}$ of that paper is equivalent to our γ or $\tan\gamma$ whence, by substitution, equation (4A) of that paper, for efficiency, becomes identical with equation (9) above.

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sharpened; in No. 3 the *blade* (as it must more properly be termed) was rectangular, and flat all over the front face, but rounded on the back like a screw blade, the mid-thickness being about one-seventeenth of the width. In both, the length was twice the width, and the length was transverse to the line of motion.

It will be seen that in Fig. 10 (Plate XX.), Nos. 1 and 2 follow each other pretty closely, as might be expected, but that No. 3 differs from the two others in a striking way in the regions of higher slip, in this respect according with the results of the screw experiments, as shown in Fig. 8.[13] This circumstance turns on a rather important theoretical point, as follows :—

In the ideal case of a screw with no edgewise resistance, where, in other words, the turning moment must be simply the virtual-velocity equivalent of the thrust, the sole waste would be slip, and the efficiency would be always = (1 − S); [S = slip ratio]. This expression, indicated by the ordinates of the straight line Z Z in Figs. 8 and 10, may therefore be regarded as the theoretical limit of efficiency, from which an actual screw must fall short in virtue of the edgewise resistance element. But it will be seen that the actual screw curves in Fig. 8 begin to trespass *outside* this line before even 30 per cent. slip is reached. The comparison between the theoretical curves for bases Nos. 2 and 3, proves clearly that this feature is incidental to the roundness of back of the blades, which must obviously operate to decrease the effective pitch (and so falsify the 1 − S line); and this not by a constant amount, since the effect at no slip has already been taken account of in the mode of assessing the analysis Pitch [see text, § 13], but by an amount which increases markedly as slip ratio increases.

In reference to Fig. 11 (Plate XX.), it should be noted that the ordinates of the curves for basis No. 3 have been calculated for the following pitch ratio values[14] :—·05, ·10, ·20, ·33̇, ·50, ·66̇, 1·0, 1·33̇, 1·66̇, 2·0, 2·4. The finish tangential to the base line at the zero pitch ratio end is consonant with theory, because, in the limiting case, for given revolutions per minute, turning moment and work expended is constant, whereas for given slip ratio both thrust and speed vary as pitch, and useful work consequently as pitch squared.

DISCUSSION.

Mr. W. J. Luke (Member of Council): My Lord and Gentlemen, when the firm of Messrs. John Brown & Co. made up their minds to establish an experimental tank, permission was obtained from the Admiralty to visit the establishment at Haslar in order that we might get some information upon its working, and it fell to my lot to be the representative who went to Haslar to interview Mr. Froude. With the inquisitiveness natural to an Englishman, I looked about the place and saw a large number of model screw propellers of a type which seemed to me rather unfamiliar, and upon inquiry I found that a very exhaustive series of propeller experiments had been carried out; I have consequently long been cherishing a hope that the result of these experiments would be published, and I think I may voice not only my own sentiments, but those of everyone who has had any dealings with this subject in thanking Mr. Froude for the very interesting and exhaustive paper he has presented to the Institution to-day. I should be paying Mr. Froude a very poor compliment if I were to profess that I had thoroughly grasped the paper in the last few days and mastered its details. It is cast in a mould that is not exactly familiar

[13] The curves in Fig. 8 terminate at zero of slip ratio, while those of Fig. 10 do not, because the latter are calculated for pitch of surface, but the former [see text, § 13] for pitch equal to travel per revolution at zero thrust, therefore necessitating zero efficiency at zero of slip ratio.

[14] The pitch ratio figures assigned to the theoretical curves in Fig. 10, and to which they are plotted in Fig. 11, are taken as two-thirds of the values proper to the actual supposed path of the plane or blade; on the supposition that the mean diameter of an ordinary screw may be taken as about two-thirds of the reputed diameter.

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to those of us who have been working with Mr. Froude's presentation of the experimental details previously published, but I have no doubt that in the course of time we shall get familiar with these experiments and the method of their use. It is to be observed that the experiments have been carried out over a range of wide area ratio, and also over a tolerably wide pitch ratio. But one is never satisfied with the amount of information given, and I am just a little sorry that Mr. Froude has not given us results covering a somewhat wider pitch ratio, not upon the lower side, but upon the upper side; because, within the last two or three years, we have had at Clydebank to adopt propellers of 1·7 pitch ratio at least. If we had a problem of that sort to tackle again, with the present published data it would mean exterpolation instead of interpolation, which is generally a delicate thing to do. As regards efficiency, it seems that Mr. Froude's experiments bear out previous results, and lead to the conclusion that the three-bladed propeller is more efficient than one with four blades; and, if I read the experiments rightly, they also show that propellers with blades of elliptical outline are more efficient than those with the wide tips. I would like to ask Mr. Froude if he can give us any information upon blades with narrow tips, and whether they turn out to be more efficient than those with the ordinary elliptical outline. And with regard to the superior experimental efficiency of the three-bladed propeller, I would ask whether these experiments are confirmed by any analysis of trial data, because in analyses of the trials that we have attempted to make at Clydebank we have had to start with the fact that some of the very best trials that we ever carried out have been with propellers with four blades. When these trials were made, there was no experimental tank at Clydebank, but since that tank has been established, the models of the vessels have been subjected to experiment, and it seems that very high propulsive efficiencies have been obtained. This causes us to wonder whether the experimental data are right, and while we cannot assert that they are wrong, it would be interesting to know if the results of practice on the large scale bear out the results of experiment. As regards these good trials with four-bladed propellers, I ought perhaps to say that they date back to a time when cavitation difficulties were not met with, but in recent practice it has become necessary to attend more closely to the provision of sufficient projected area to satisfactorily give off the requisite thrust. It may be, therefore, that in some cases a four-bladed propeller may be more suitable than one with three blades.

It is also very gratifying to find what seems to be the result of experience thoroughly confirmed by experiments in the tank—that is, that only indifferent efficiency can be obtained with a propeller of low pitch ratio. This may possibly bring us again to the consideration of gearing, as was suggested by one of yesterday's speakers. Mr. Froude compares his experimental results with the theoretical expression for efficiency—viz., $e=1-s$. In a contribution to the American Society of Naval Architects by McEntee in 1906,* the conclusion is reached that the proper expression for efficiency is $\frac{1-s}{1-\frac{1}{2}s}$. In thinking the matter over, I was rather disposed to agree with the latter view of the case, more particularly as it seemed to be led up to by Mr. Froude's own contributions to this Society, upon the part played in propulsion by differences of fluid pressure, and I hope Mr. Froude can enlighten us a little upon that point.

The Hon. C. A. Parsons, C.B., M.A., F.R.S., D.Sc. (Member of Council): My Lord and Gentlemen, the subject of this paper is so complex that it is very difficult to do justice to it on short notice, and I came rather to listen than to speak. I would, however, like to ask Mr. Froude one or two questions. I do not find in the paper any reference to corrections for scale in the case of applying the results of the model experiments to full-sized propellers of ships. If a naval architect is designing a propeller for a large ship, what co-efficient of correction for size should he take? We all know that in deducing a ship's hull resistance from model experiments, there is a co-efficient for size which was first established by the late Mr. William Froude, and which affects the result as applied to full-size vessels very considerably. The skin friction on a small scale body is greater per square foot than that of a large or long body. Now, in a screw propeller one would think that this correction would be a very large factor. If we take the view

* Trans. Soc. Nav. Arch. and Mar. Eng., Vol. XIV. p. 85.

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of the propeller question that the losses are made up of two principal quantities, one due to the loss by slip, and the other by the skin friction of the blades through the water, we might generally assume—I speak, of course, under the correction of Mr. Froude on this subject—that when the two are somewhat equal we get the maximum efficiency, according to the laws of maxima and minima, and this I have found to hold approximately, with certain obvious reservations, in propellers of ships. Then, in the case of wide-bladed propellers, the skin friction is a much larger factor than it is in narrow-bladed screws. The narrow-bladed propeller for moderate-speed vessels and moderate thrusts per sq. ft. is undoubtedly slightly more efficient than the wide-bladed; but if we try to use the narrow blades for a fast ship with high pressures per sq. ft. of blade area, we get cavitation, and the result is a failure.

A question which has often been raised by naval architects in recent years is, What is the difference in efficiency between a wide-bladed screw of, say, 1 pitch ratio and the best ordinary screw; the wide-bladed screw—covering, say, 60 to 70 per cent. of the disc area by its projected blade area—and the ordinary screw covering from 35 to 40 per cent.? I have been trying to work out the difference, but I am afraid I have not had time and am not sure that I have quite mastered the curves in the paper, but the difference, I think, appears from them to be about 6 to 7 per cent. Then again the question arises, Can we deduce from model experiments a true inference as to what we should get in a large ship? We have found in practice—and I think it has been the experience of all those who have fitted high-speed turbine screws—that a three-bladed is more efficient than a four-bladed; is this difference wholly due to the co-efficient to which I have referred?

Then, of course, with regard to the question of cavitation, one may get an excellent model screw in a tank and try it on a fast ship, and find it is a failure; cavitation and the scale of size are probably the cause. If the back of the propeller is very much rounded, this may cause cavitation (even though the propulsive pressure on the blades is not excessive), and if cavitation arises we are very apt to get corrosion. We first observed a very remarkable result of cavitation with tandem screws on the shafts of one of the earlier turbine destroyers. The after screws became pitted, and this was attributed to the fact of the after propeller cutting the cavities in the water produced by the blades of the forward propeller. It seemed as if, when the vacuous vortex cavities were produced by the blades of the forward propeller, some gas was extracted from the sea-water, and when those cavities were cut and closed up by pressure from the blades of the after propeller, they deposited some acid on the pressure side of the blades, which corroded them over such a part of their surface as crossed the stream lines of the cavities, the corrosion being over the face of the blade, and not on the leading edge.

Professor J. H. Biles, LL.D. (Vice-President): My Lord and Gentlemen, this paper illustrates the loss we have suffered in past years by having so few experimental tanks, and it brings to our minds again the kindness of Mr. Yarrow's proposal. A great deal of the work described in the paper has taken Mr. Froude a good many years to do, owing to other work which was pressing. If we had had our research tanks, and supposing we could find someone who could do the work anything like as well as Mr. Froude, we should perhaps have had these results before. It is impossible, of course, for any one of us to master this paper by merely reading it two or three times. My experience of the papers which Mr. Froude has given us from time to time is that it is almost necessary to teach them before one can learn fully what is in them. The subject is so deep, and the papers contain such a mass of information, that it takes a very great deal of time to really find out the enormous value that there is in them; and the Admiralty deserve the thanks of us all for allowing Mr. Froude to place the results before us in such a complete form.

The object of this paper is partly scientific and partly practical, and I should like to ask Mr. Froude if he could possibly give us some idea of the character of the reliability of the results of prediction for full-sized ships based upon these model experiments. Perhaps he could also say approximately how the efficiencies of full-sized propellers at sea compare with those of model propellers? A young man in an institution the other side of the Border recently gave us a dictum on the subject, that no screw could possibly have an efficiency of more than 70 per cent. Perhaps Mr. Froude would say whether such a statement is correct? With reference to the relative efficiency of four and three-bladed screws, it is

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remarkable how very few three-bladed screws there are in use in single-screw ships. The almost invariable practice is to put four-bladed screws into single-screw ships. I do not know whether the position of a single screw and the frictional wake in any way interfere with the relative efficiency of the three and four-bladed screws in cases where Mr. Froude's experiments show that the three-bladed screw is more efficient than the four-bladed. With regard to the question of whether we are always to be bound by the direct connection of the propelling engine with the propeller, I think it must be obvious that if we can make our revolutions of propeller just what we like, and not necessarily those of the engine, we shall be much more likely to choose the most efficient propeller, and not be compelled, as Mr. Parsons has pointed out we have to do now, to adopt propellers of abnormal area in order to avoid the cavitation difficulty. If the size of the propeller is fixed independently of the revolutions of the engine, we shall be able to adopt what will not only be the best proportions of the propeller, but the best areas of propeller.

Mr. C. E. STROMEYER (Member of Council): My Lord and Gentlemen, it is always with great expectation that I look forward to papers on tank experiments, but they generally leave one with a feeling of disappointment. This paper, for instance, must contain the results of an enormous amount of valuable experimental information, and we have heard from one of the speakers that he has seen the work that was done, but all that we get this afternoon are mathematical deductions, and I think many of us would very much have preferred the actual experimental data. This is practically what I said when the last paper on this subject was read; and then, as now, I complained that the speeds were very low. Even now the experiments have only gone up to 3 knots, whereas some propellers actually travel at the rate of, I believe, 36 knots; that means that the horse-power per sq. ft. of area of actual propellers is over one thousand times greater than that of the experimental ones, and to jump from the results which one gets with a power of one to the conclusion as to what may happen at a thousand times greater power is, to my mind, a rather doubtful proceeding, and that is another reason why I should very much like to see the details of these experiments. When the last paper on these experiments was read in 1886, I suggested on this very point that the speed could be increased if the conditions were reversed. I gave a sketch* of what I meant, and the formula which I had found to represent the normal pressure, and now that a new tank is to be built at the National Physical Laboratory I do hope that arrangements will be made there to deal with this important subject. Very much more powerful gear would have to be provided than is found in the tanks that are now existing, but I do think that the question of propellers is such an important one, with the present high speeds and turbines, that it would be desirable if something in that line could be done, and I look forward to that in the future.

Fleet Engineer GEORGE QUICK, R.N., ret. (Visitor): My Lord and Gentlemen, may I venture to ask a question—viz., Is the efficiency of the wing screws of the great Cunarders equal to or superior to that of the middle screws? In the case of triple and quadruple screw ships it would be very interesting to know what has been the true efficiency of the wing screws as compared with that of the middle screw or screws. I am much interested in this matter of multiple propellers, as I wrote in favour of the adoption of triple propellers for large ships of war upwards of thirty years ago. When I investigated the results of the trials of the United States cruiser *Columbia* (the first large triple-screw ship built), I found, according to my calculations, which may have been primitive, that the middle screw appeared to be more efficient than the wing screws. Consequently I have tried to find what differences should be made in the details of the wing screws and the middle screw in order to obtain the maximum efficiency from the whole power of the engines combined. I think we might expect to get much greater efficiency if we indulged in more enterprising—or, I might call them, more imaginative—experiments. At present, we restrict ourselves rather too much to the orthodox course of experimenting with those forms of propellers which we already have, in order to ascertain their exact value, rather than to discover new forms of greater value. By virtue of the experiments which have been made, we know what the three-bladed screw will do and what the four-bladed screw will do, when these are of certain definite forms, but

* Trans. I.N.A., Vol. XXVII. p. 277.

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I submit that it would be well worth while to travel sometimes outside these forms. I have myself gone so far in carrying out a special design as to make a screw which was successful in knocking off 30 per cent.* of the efficiency as compared with the common screw. I think others have done something similar to that before. I wanted to test certain principles, but those principles cannot be tested by making propellers according to the ordinary form, and I would like to see a few experiments made with forms very different from those in ordinary use to-day. With reference to the new departure which we now have of turbines running at very high speed and driving four propellers, with the wing screws—comparatively speaking—very close to the ship, and with the middle screws deeply immersed, I think a vast field is opened up for improvement by a radical change in the general design of propellers for high-powered ships, and by differentiating the design of the wing screws from that of the middle screws.

Mr. J. H. Narbeth, M.V.O. (Member): My Lord and Gentlemen, this is undoubtedly a paper of great value, but, as Mr. Luke has told us, it will take us some time to practically investigate the information given and find out the whole of its worth. I have no doubt that when we have had time to do so, it will be found to be, like the previous corresponding paper in 1886, of historical importance. There is a consensus of opinion that model experiments on screws are necessary, and that they will be still more necessary in the future. Model screw experiments require tanks and very elaborate apparatus, and they are very costly; but experiments on actual screws of ships are more costly still, and a thought has been running through my mind which may be of some service in reducing this cost. I do not suppose that the idea is original, although I have not found it dealt with anywhere. Turning to paragraphs 10 and 11 of the paper (page 187), we find that for simplification of analysis, Mr. Froude practically refers to a base A C E (see diagram, paragraph 11) instead of the drawn line at the bottom. This means that the slip ratio of the screw is taken as the basis of consideration rather than the pitch ratio. Any point on the line A C E would correspond to advance without slip. If we look at the small diagram at the bottom of the same page, we see that the lower line is drawn at right angles to the axis of advance; the upper drawn line represents the path an element of the screw would travel over if it had no slip, and the dotted line represents the actual travel. The estimated thrust which the screw gives is derived by considering what happens when an element is moving from a position on the upper drawn line to a position on the dotted line; that is to say, the thrust is dependent on the slip angle, which is the angle between the upper drawn line and the dotted line. It has occurred to me whether it would not be possible to simplify the actual experiments somewhat in the same way as Mr. Froude has simplified the analysis, by making experiments on models of screws in which the pitch angle did not correspond to the pitch angle of the actual propeller, as is now the rule, but to the estimated slip angle. The model screw would then be run without any advance, the

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* With reference to the loss of 30 per cent. of efficiency above referred to, I have records of many trials of propellers which have come under my observation. The trials were made on a boat of 200 I.H.P. The following is a short extract from a Table of Comparison of Results:—

No.	Relative Max. Speed.	Relative Efficiency.
1. Propellers of highest speed	1,000	1,000
2. „ „ „	997	941
3. „ „ „	979	953
4. Special design	966	1,070
5. Increasing pitch	956	1,041
6. Special design	955	1,041
7. „ „	950	1,053
8. „ „	925	1,032
8. „ „	925	1,032
Average of propellers not included in the above	941	950

The above shows that the propeller which gives the highest speed is not necessarily the most economical at that highest speed as compared with the economy of another propeller of lower maximum speed; but if the speed of propeller No. 1 were reduced to that of No. 4, the efficiency of No. 1 may be found to be as great as or greater than that of No. 4.

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apparatus for travelling the screw forward would not be used, and the means of driving the model and measuring results would be simplified. If this is practicable, very much larger model screws might be used, and a much nearer approximation to actual results obtained. I have not been able to find anything to show to what extent errors would arise in this process, and I do not suggest that the method would replace systematic series of experiments, nor that it is by any means free from objection. I mention the matter now in the hope of obtaining some expression of opinion as to whether the method is likely to be of any practical value in differentiating between screws which are nearly alike, and so saving much of the expense of making variations in full-sized screws and trying them in succession on actual ships.

Mr. C. Tennyson (Member): My Lord and Gentlemen, I would like to make one remark in connection with this highly interesting paper—as all papers by Mr. Froude are—referring to a special case of the general question of screws—viz., to the question of several screws on one shaft. There are very few data published in connection with this question—at least, from practical experiments—and I know of only one paper on this subject. This is a paper in the Transactions of the French Association Technique* Maritime by Monsieur Doyère, who at that time was fully convinced that two screws and, in general, several screws on one shaft would be in many cases much better than one single screw. Of course, I also know that since that time there have been tandem screws, as they were sometimes called—that is to say, two screws on one shaft, used in several ships having turbine engines—and that it was found to be more advantageous to divide up the engines and have a greater number of shafts and only one screw on each shaft. This may be generally so, but I think there are some special cases when good results would be obtained if you used two screws on one shaft, for example, in very shallow draught ships of great speed. We hardly possess such ships at present, because even if the ship's hull draws very little, the screw always draws a great deal more. With the remarkable results which Mr. Yarrow has achieved with his motor boats, you will see that when the hull draws comparatively little, perhaps not more than 1 ft., the lower tip of the screw blade draws about 3 ft. or something like it. Now, in some cases it would be highly desirable that we should be able to design a small ship of very light draught and of great speed at the same time, and this should favour the use of a shaft with two screws on it. With such a ship, it would hardly be possible to use tunnels and to have the screw of greater diameter than the draught of the ship, and experiments in this direction would, I am sure, be highly desirable. Such experiments would give us the necessary information as to the best elements (diameter, pitch, &c.) to be chosen for each screw in different cases, and as to the necessary distance between screws fitted on the same shaft.

Mr. J. E. Wilkins (Member): My Lord and Gentlemen, I have been extremely interested in this paper, and the remarks that have followed it. Under the head of Design, page 190, Mr. Froude remarks, with reference to the x y curve: "For design, where it is generally desired to calculate propeller dimensions suitable to given horse-power, revolutions, and speed, a difficulty arises from the fact that diameter, an unknown quantity, enters into both x and y." Some years ago, I collected a very considerable quantity of trial trip data, relating to vessels of very diverse types, which data included particulars and dimensions of their propellers, and I carefully examined the speed performance of each, both in relation to propeller dimensions and also the piston speed relatively to speed of ship, which led me to formulate certain rules, and I would like to state what my practice has been when determining propeller dimensions for—if I may say it—the considerable number of successful steam yachts which I have designed. I do not know whether the practice obtains at all amongst engineers or shipbuilders, when calculating propeller dimensions, to proportion the diameter of the propeller to the displacement or the breadth of the ship, but it seems to me it is only rational to do so, and it has been my practice to make the diameter of the propeller proportional to the displacement and breadth of the vessel combined. If the sixth-root of the displacement, expressed in cubic feet, be multiplied by the square-root of the breadth, and the product divided by the propeller diameter, it will be found that the quotient varies from about 2·5 to 3·0—that is,

* Bulletin de l'Assoc. Techn. Mar., 1900, p. 261.

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for a single propeller. Personally, I prefer a four-bladed propeller to a three-bladed propeller, and I have found a pitch ratio of 1·33 very suitable for a single screw having four blades. With regard to piston speed relatively to speed of vessel, I always estimate that the piston velocity in feet per minute should be forty times the velocity of the ship, in knots, and, further, that the stroke of the piston should be one-sixth of the propeller pitch, so that there is a correlation in regard to ship, engine, and propeller. It can be shown that the speed of ship varies directly as the piston speed, and that generally a piston speed of 40 ft. per minute is required for each knot that the ship is to be propelled. Thus, the engines in a 32-knot torpedo boat would have a stroke of piston of about 18 in., and would run from 420 to 430 revolutions per minute, or, say, 1,280 ft. piston speed per minute. If the piston speed is less than 40 ft. per minute per knot, then the general efficiency is greater, and if it be over 40 ft. per minute per knot the efficiency is less.

The Chairman: Mr. Froude suggests that your remarks are not quite relevant to the subject of the paper.

Mr. Wilkins: Then, my Lord, I will not add anything further.

Mr. H. Wheatley Ridsdale (Member): May I ask Mr. Froude one short question? In page 190, under the heading of "Design," Mr. Froude says: "The x y curve, with its companion efficiency curves, serves perfectly for analysis of steam trials." I would like Mr. Froude to explain in his reply, if he would be so kind, how he would deal with the question of wake correction?

Mr. R. E. Froude, LL.D., F.R.S. (Honorary Vice-President): With regard to Mr. Ridsdale's point, of course we are always obliged to put a value on the wake in some way in the analysis, and we treat it, provisionally at least, as a known quantity based on model experiments. We generally have our wake determined from the model, but if not, we have to estimate it from parallel cases as nearly as we can. As regards the relevance of Mr. Wilkins' remarks, what I feel is that the paper does not profess to be a treatise upon screw efficiency in general, but simply a description of a series of experiments and the data which result from them. If we were to discuss all the questions that bear upon screw efficiency, we should be here till midnight! The remarks of other speakers I will take in converse order. Mr. Stromeyer objected that these screws are very small compared with actual screws, and that the speed is very low. Of course, that is so. I think we should all of us wish, if we could, to make our experiments on full size, but of course there are difficulties in the way of doing so, and we do the best we can on a small scale. What I would point out is that if you use, as we must use, small screws, you must also use a low speed, on grounds of similarity. In point of fact, however, we have experiments on model screws of various sizes, within the range of the small dimensions that we use, and varying speeds also, and we find, broadly speaking, that neither the scale nor the speed makes a very material difference. Again, the comparisons which we have with ship trials seem to show that, at any rate, our results are not very far wrong. Professor Biles asked how far our results are verified by steam trials. Of course, in one important respect—I mean the relation of the revolutions to the thrust or the power—the verification is always difficult, owing to uncertainty as to the precise effective pitch. We certainly find, for instance, by comparison of trials of the same ship or of sister ships, with different propellers, that you cannot depend upon the reputed pitch to a great nicety, and therefore we are obliged to take the pitch in ship trials more or less as an unknown quantity. That, of course, makes the verification difficult, in that it removes one element of precise test; but as regards efficiency, we have had cases of trials of a ship or sister ships with screws of very various types, and have found that the results, on the whole, did bear out the variations of efficiency which our model experiments had predicted. However, verification by steam trials is always a very difficult thing, because there are so many unknown variants. The engine efficiency varies, and the speed, again, is a difficult thing to get with accuracy; but I should say, as far as we can see, our results are as well verified in full scale as we have any means of determining them.

Professor Biles: Do you mean absolutely or relatively to each other in efficiency?

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Mr. FROUDE: Relatively to each other in the first place. Absolute verification is the most difficult of all to get.

Professor BILES: Are the efficiencies absolutely verified?

Mr. FROUDE: We do not know precisely what the efficiency of a full-sized screw is, in a ship, because there are so many unknown elements of loss, but the efficiency which we predict for a screw accounts for the whole loss of work in a ship very fairly well, as far as one can see. Professor Biles spoke of the efficiency limit of 70 per cent., as laid down on theoretical grounds. In our models we have more than that. We get, I think, 73 or 74, so that I do not think you can lay down any theoretical law of that sort. Speaking of the effect of difference of scale which Mr. Parsons referred to, I should think no doubt there must be something of the nature of skin friction correction which should be applied to screws as well as to hulls. There is every reason to expect it, but the effect should be in the direction of giving higher efficiencies for large screws than for small. Now, when you see what high efficiencies we do get with our small screws, there is not room for any large difference in that respect between small and large scale. That seems to me to be an important consideration. Mr. Luke asked whether we had tried narrower tips as well as wider tips. You will see that the difference between our wide tips and our elliptical blades is very considerable, and the difference in efficiency is, after all, so very moderate that I should say the general conclusion to be drawn from our experiments is that within very wide limits you may vary the shape of your blade a very great deal without substantial difference in efficiency. So, again, as regards the question of three blades *versus* four blades. The difference, as we are able to show it, is certainly nothing very great. I confess I do not attach very much importance to it. We have conditions in the model which, of course, are different from those in the ship. For one thing, you have always in the ship a much bigger boss than we have in the model, and I think it is quite possible that when you have a bigger boss it may be more worth while having a fourth blade than it is in the case of a model where you have such a little boss. But it must be borne in mind that you cannot have it both ways. If you want to largely increase the total proportion of blade area to disc area, you can do it either by having very wide blades or by putting four blades instead of three, but you cannot usefully do both; and our experiments show that when you come to a low pitch ratio, the combination of wide blades and four blades is very prejudicial to efficiency. I do not think there are any more points that need be referred to. The question of the $(1-s)$, which Mr. Luke referred to, is purely a question of arithmetic. You have certain speeds, forces, and directions, and these result in such and such an equation. We are taking the slip as given by the revolutions. The proposition to which Mr. Luke has referred in a previous paper of mine related to the amount of reaction that you could get out of a given column of water. That is quite a different question. I am not here taking any account of the water. I am simply supposing the screw to be moving at a certain number of revolutions and to have such and such forces acting upon it, and from that expression of efficiency the $(1-s)$ expression follows as a mere matter of arithmetic.

The PRESIDENT (the Right Hon. the Earl of Glasgow, G.C.M.G.): I am sure you will all join with me in giving Mr. Froude a hearty expression of our thanks for his admirable paper.

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AN ANALYSIS OF THE RESISTANCE OF SHIPS.

By Professor WILLIAM HOVGAARD, Member.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 10, 1908; the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D., President, in the Chair.]

INTRODUCTORY.

THE wave-making resistance, or, as it shall here, for the sake of briefness, be called, *the wave resistance*, is conceived by the author as depending essentially on three factors: the speed, the dimensions (understood in the sense explained below), and the fineness.

Consider first a given ship, driven at different speeds. Its wave resistance may be expressed thus:—$R_w = \beta f_1(V)$, where V is the speed in knots, $f_1(V)$ is a function depending essentially, if not exclusively, on speed, and β is a co-efficient depending essentially on size and form of ship. Let R_w be given in pounds.

Now imagine all the ordinates parallel to one of the principal axes of the ship to be altered in the same proportion, a process which on a line drawing may be effected by a simple change of scale parallel to the corresponding principal dimension. This would not affect the fineness, nor, indeed, the general character of the lines; but it would affect the size and form of the ship, and hence also the resistance.

Let L = Length of ship on water-line in feet.
B = Extreme breadth on water-line in feet.
D = Mean draught in feet.

Then, since by the transformation here proposed, all the ordinates parallel to any one of the principal axes preserve the same proportion to the respective principal dimension, the term of the resistance, which for a given parent type expresses all such changes, must be some function of L, B, and D. Hence it is proposed to write—

$$R_w = b f_2(L, B, D) f_1(V),$$

where b depends on the fineness and character of the lines, *i.e.*, on all the features of form which are independent of changes of scale. Let δ stand for a typical or characteristic co-efficient of fineness, then—

$$b = f_3(\delta) \text{ and } R_w = f_3(\delta) f_2(L, B, D) f_1(V) \qquad (1)$$

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This separation of the elements, which influence the resistance, into three factors forms the basis of the present analysis.

It is known that the formula for wave-resistance contains a periodic term expressing the effects of the interference of the transverse bow and stern wave systems. The scope of this paper is, however, limited to a study of the wave resistance apart from interference, and, since the periodic term thus omitted is alternately positive and negative, the curve of resistance, which it is here attempted to express mathematically, will, in a sense, represent a mean or average of the resistance.

It is not here asserted that the three factors of the expression for R_w are entirely independent of each other, but it is the object of the following analysis of experimental results, combined with certain theoretical considerations, to examine this point, and, if possible, to find an approximate and simple form for the three terms.

In order to determine the co-efficients that enter into the expression for the resistance, it has been found necessary to proceed so as always to have only two unknown co-efficients in the expression. It thus becomes possible to use the method of focal diagrams described in Appendix I., by which the approximate solution of several simultaneous linear equations with two unknown quantities may be conveniently found.

Hence *the total resistance* has been expressed by means of two terms, one of which represents the resistance due to surface friction, the other comprises the wave resistance, together with the eddy resistance and minor resistances; in fact, all that is usually referred to as *the residuary resistance*. It is assumed that the former term varies as $V^{1\cdot825}$. The use of a somewhat different exponent, such as 1·854, which has been used in this paper in case of some of the Washington experiments, does not affect the conclusions to which the present analysis leads; its chief effect is a slight change in the co-efficient of friction. It is further assumed that the residuary resistance, which we shall denote by R_r, may be expressed in the same form as R_w in equation (1).

Thus the expression for the total resistance becomes—

$$R = a V^{1\cdot25} + f_3(\delta) f_2(L, B, D) f_1(V) \qquad (2)$$

The co-efficient a, as determined by the focal diagrams, has been further analysed by dividing it by the wetted surface S (in square feet), giving a co-efficient f, which may be compared with the experimental co-efficient of friction.

Some Remarks respecting the Formation of Ship Waves.

Naval Constructor D. W. Taylor, U.S.N., has recently* given the results of some interesting experiments on the flow of stream lines around the bottom of ship models. Figs. 2 and 3 (Plate XXV.) give two of Mr. Taylor's diagrams, which seem to show that

* Trans. American Society of Naval Architects and Marine Engineers, 1907.

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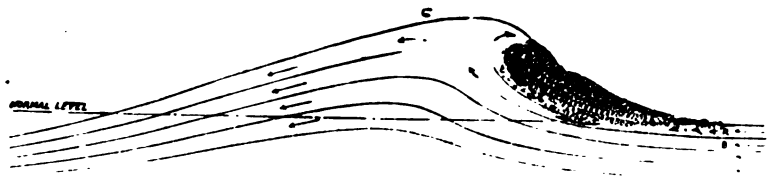
in meeting the ship the surface water at the bow, down to a certain depth, flows out sideways in nearly horizontal planes, while for the lower strata the stream lines, after having formed an upheaval at the bow, dive down underneath the bottom, running in more nearly vertical planes. Emerging under the quarter of the ship, the lower stream lines form here a second upheaval, similar to and often as pronounced as the forward upheaval. The dividing region between the upper and the lower layers is at the round of the bilges, and seems to be well defined.

Judging from these experiments and also from personal observations of ship waves, the author has formed the following conception of the flow of the upper layers of water near the bow.

SKETCH ILLUSTRATING THE SUPPOSED FLOW OF THE STREAM LINES IN THE UPPER LAYERS NEAR THE BOW OF A SHIP.



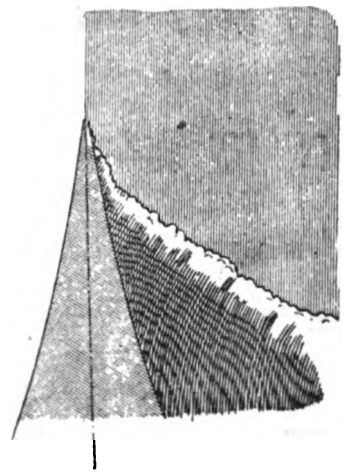
NOTE.—The stream lines of this diagram are supposed to be originally in a vertical longitudinal plane, which intersects the breaker of the forward upheaval at a small distance from the ship, and the stream lines are shown in projection on this plane Arrows in the eddies indicate the general direction of flow of eddying water.

The ship, and therefore the wave, is assumed to be stationary, and the water to be flowing past it with the velocity of the ship.

The stream lines of the upper layers flow somewhat upwards over the upheaval formed by the lower layers, and are at the same time deflected outwards and retarded in speed. By their expansion they serve to accentuate still more the upheaval formed by the lower stream lines. Having reached a certain height above the normal level, the stream lines nearest the surface will, owing to their retardation in speed, fold themselves back over the free surface, whereby the continuity of flow is broken and a breaker is formed. Thus the energy of the surface layer A is partly destroyed by eddying; ultimately this layer unites with the water in the layer B next below, and emerges at C. The breaker extends outwards from a point a little aft of the stem in an oblique direction, forming an angle θ with the transverse axis of the ship.

In the triangular space left between the breaker and the ship the water is seen to be clear, dark, and unbroken, with a glossy surface; it is here that the lower stream lines of the upper layers emerge, whence they flow on to the first hollow of the diverging waves. (See Sketch.)

As the speed increases, the upheaval and the permanent breaker so formed increase in height, and the breaker may in ships with a ram bow form a veritable



SKETCH OF BOW WAVE SEEN FROM ABOVE.

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cascade. Although the resistance caused by the maintenance of this breaker must be very considerable in many ships, it has not, as far as the author is aware, generally been taken into account as a separate feature in the theory of resistance.

It appears, however, that the energy of the stream lines nearest the surface which is thus, at least partly, consumed in the breaker, should be considered apart from the energy, which is lost in wave motion, since it is initially and definitely lost. The phenomenon resembles the action of a jet meeting a solid surface, and it seems likely that the resistance connected therewith is proportional to V^2 and to the square of the linear dimensions of the ship. It may thus properly be merged with the ordinary eddy-making resistance, and may, like this latter, be assumed to follow the law of mechanical similitude.

The forward and aft upheavals each give rise to a system of waves, the most prominent features of which are known to be a series of transverse and a series of diverging waves. Thus the energy which is present in the forward upheaval is continually dissipated in three different ways: by the breaker, by the transverse waves, and by the diverging waves; but by the action of the ship, in overcoming the resisting pressures, this energy is constantly renewed. The action of the aft upheaval is probably similar in character, but different in degree.

Considering that the upper layers are deflected horizontally, whereby the particles, besides their elevation above the normal level, are given an oblique outwards motion, it seems not unreasonable to conjecture that this deflection of the stream lines of the upper layers is the main cause of the formation of the diverging waves. Further, it seems likely that the deflection of the stream lines of the lower layers in nearly vertical longitudinal planes is the main cause of the transverse waves, although, probably, the two actions cannot be considered entirely independent of each other.

The Speed Term.

The wave resistance is equal to the energy communicated by the ship to the wave system per unit time divided by the speed of the ship. Hence the wave resistance connected with the *transverse waves* is generally supposed proportional to

$$\frac{\lambda_0 h^2 l}{V\left(\frac{l}{V}\right)} = \lambda_0 h^2,$$

where h is the height of the waves, l is the length of waves normal to the crests and λ_0 is the effective, or virtual, breadth along the crest line. It is usually assumed that $h \propto V^2$, and that λ_0 is practically independent of the speed. Although this latter assumption has not, as far as the author is aware, been scientifically verified, there appears to be no evidence to disprove it, and it may be accepted here as approximately correct. We shall, therefore, assume this part of the resistance proportional to V^4.

The *diverging waves* appear to be parallel to the above-mentioned breaker. The velocity of this breaker, normal to itself, is $V \cos \theta$, and along the direction of its crest

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it is V sin θ. Hence the diverging waves which are generated directly by the remaining energy of the breaker are created on both sides of the ship at the rate V sin θ. The energy per unit length of the diverging waves is proportional to $h^2\ l\ \cos^2\theta$, where l, as above, is the length of a wave travelling at same speed as the ship, and the resistance is therefore proportional to

$$\frac{h^2\ l\ \cos^2\theta\ \mathrm{V}\ \sin\theta}{\mathrm{V}} = h^2\ l\ \cos^2\theta\ \sin\theta.$$

It is generally believed that the angle θ varies but little at different speeds, even in different ships; and, since both h and l vary as V^2, we may conclude that this part of the resistance varies as V^6.

This conclusion is borne out by the diagrams of wave systems given by Mr. R. E. Froude in 1881,* which give the wave systems for two similar ships, going at the same speed, 18 knots; the linear dimensions of one being four times the linear dimensions of the other. On these diagrams the diverging waves appear to be almost identical in the two ships, *i.e.*, they are independent of the size and linear dimensions; but, if this is the case, the resistance due to these waves must likewise be independent of the linear dimensions, and, therefore, according to the law of similitude, proportional to V^6.

The expression for the wave resistance proper, excluding that corresponding to the breaker at the bow, would thus be of the form—

$$\mathrm{R}_w = p'\ \mathrm{V}^4 + q'\ \mathrm{V}^6 \qquad (3)$$

where p' and q' are co-efficients, and the expression for the total resistance would become—

$$\mathrm{R} = a\ \mathrm{V}^{1\cdot825} + r'\ \mathrm{V}^2 + p'\ \mathrm{V}^4 + q'\ \mathrm{V}^6 \qquad (4)$$

where $r'\ \mathrm{V}^2$ comprises ordinary eddy resistance besides the resistance due to the breaker at the bow and minor resistances.

As stated above, the residuary resistance, comprising the last three terms of equation (4), may in the following analysis be expressed by a single term, varying as V^n, and by a tentative process we may attempt to determine the value of n, which gives the nearest approximation to the experimental results. The following considerations are useful as a guide in the choice of the value of n.

Suppose first $n = 4$, *i.e.*, we substitute for equation (4) the formula —

$$\mathrm{R}_4 = a_4\ \mathrm{V}^{1\cdot825} + \beta_4\ \mathrm{V}^4 \qquad (5)$$

where the suffixes follow the exponent of V in th last term, and where $a_4 = f_4\ \mathrm{S}$.

Comparing (4) and (5), it is seen that the V^2 and the V^6 term have disappeared in (5), but it will be found that the sum of a V^6 term and a V^2, or $\mathrm{V}^{1\cdot825}$, term may, up to a certain value of V, be represented very approximately by a V^4 term by a proper choice

* Transactions I.N.A., 1881, Plate XVII. (Fig. 4).

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of co-efficients. It is impossible by means of a focal diagram to distinguish between a real V^4 term and such a fictitious V^4 term, except at very high speeds. The focal diagram simply indicates, by its more or less perfect intersection of the lines, the values of α_4 and β_4, which most nearly make R_4 equal to R within the speed-range of the given curve. If, then, f_4 is found to have the same value as the experimental co-efficient of friction, f, and if the intersection of the diagram is good for all speeds, we may conclude that the V^6 and the V^2 terms, if they exist, have matched each other so as very accurately to make up a V^4 term, and equation (5) may be used to represent the given curve of resistance within the given limits of speed. If the V^6 term is very great, so as to require for its transformation into a V^4 term more than the V^2 term, it will absorb part of the $V^{1.825}$ term, and we shall find that f_4 is smaller than the normal value of f. This may, therefore, be expected to be the case where the diverging waves are very prominent. If, on the other hand, the V^6 term is small, it will only absorb part of the V^2 term, the remainder of which will be found in the $V^{1.825}$ term, causing an augmentation of f_4.

If we make $n = 6$, *i.e.*, if we analyse according to the formula—

$$R_6 = \alpha_6 V^{1.825} + \beta_6 V^6 \qquad (6)$$

the V^4 term of (4) will for its transformation into a V^6 term require the subtraction of a V^2 or a $V^{1.825}$ term. Writing (4) thus—

$$R = a V^{1.825} + r' V^2 + q' V^6 + (p' V^4 - r'' V)^2 + r'' V^2,$$

where, approximately,

$$p V^4 - r'' V^2 = q'' V^6$$

we see that this transformation causes an addition to the terms of low power, and the value of f_6 may therefore always be expected to be greater than the normal value of f. We have thus in the value of f_4 and f_6 some indication as to the form of the more complete formula, and hence also as to the true nature of the resistance. The available curves of resistance have on the basis of these considerations all been analysed by focal diagrams both for $n = 4$ and $n = 6$. The values of α_4, β_4 and α_6, β_6 have been selected from the focal diagrams with due regard to the effects of wave interference and to the experimental errors at very low speeds.

Thereafter the corresponding parabolic curves representing R_4 and R_6 have been constructed and compared with the experimental curve. Eventually the values of α and β have been modified until the best possible correspondence has been attained both for R_4 and R_6.

On Plates XXI. to XXIV. the more important sets of curves are given, and a few focal diagrams are shown as illustration of the method. Each set consists of three curves, denoted by R_4, R_6, and R, the latter being the experimental curve. The abscissæ give values of V and of $\frac{V}{\sqrt{L}}$, this latter ratio, which is often in the following denoted by c, being given in order easily to identify the irregularities of the R curve due to wave interference.

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In Appendix II. a detailed description of the analysis will be found; it need only be stated here that, as a general result of the analysis, the curves for R_4 and R_6 appear generally to give equally good correspondence with the experimental curves R, as may be seen by inspection of the curves. The apparent better correspondence of R_6 at speeds from about $c = \cdot 75$ to $c = 1 \cdot 25$, where R_4 in most cases falls lower and further from R, may generally be explained by the presence of a hump in the R curve, due to wave interference, but some cases are found where R_6 gives decidedly the best result (Figs. 4 and 11, Plate XXIV.). On the other hand R_4 shows a much better correspondence than R_6 at very high speeds, $c = 2$ to 3 (Fig. 10, Plate XXI., and Figs. 9 and 10, Plate XXIV.).

It would not be possible, therefore, on the basis of the curves alone, to decide whether the formula for R_4 or that for R_6 gives in general the best approximation to the experimental curves. It is only by considering the values of the frictional co-efficients f_4 and f_6 that we can obtain an answer to this question. As seen from Table I., the value of f_4 corresponds throughout fairly with Froude's co-efficients of friction f, which are given for comparison, while f_6 is almost invariably considerably larger than f. In cases where the given R curves represent residuary resistance, it is in accordance herewith found that $f_4 = 0$, or is very small, while f_6 has an appreciable value.

It appears, therefore, that if the form of the complete expression for the resistance is as given in equation (4), the V^6 term is on the whole fairly matched by the V^2 term so as to combine to form a V^4 term, which merges into the original V^4 term. In ships of full form a reduction of f_4 below the normal value of f is observed in several cases (models of *King Edward* type, *Greyhound*, &c.); this is presumably due to a greater prominence of the V^6 term, *i.e.*, of diverging waves, but the material at hand is too limited to enable a definite conclusion to be drawn on this point.

It is concluded, therefore, that the equation—

$$R_4 = f_4 S V^{1 \cdot 825} + \beta_4 V^4 \qquad (5)$$

gives in most cases a fair approximation to the actual mean curve of resistance.

It will be noticed that in all cases the experimental curve of resistance falls off very considerably at about $c = 1 \cdot 75$, as might be expected, since at this point the hollow of the first bow wave begins to neutralise the crest of the first stern wave. In case of destroyer models (Figs. 9 and 10, Plate XXIV.) the curve seems to rise again to the steepness of the R_4 curve at about $c = 2 \cdot 5$, but whether the same feature would be found in ships of different type the author has not been able to determine, in the absence of the necessary data. In case of some full ships the experimental curve shows a marked falling off already at $c = 1 \cdot 25$, a fact which can hardly be explained by wave interference alone. (Fig. 10, Plate XXI.)

The Term Involving the Principal Dimensions.

If we change the scale parallel to all three principal axes in the same ratio, *i.e.*, if we pass to a ship of similar form, the residuary resistance will, according to the law

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of mechanical similitude, be proportional to the cube of the linear dimensions at speeds proportional to the square root of the linear dimensions. This law applies to the residuary resistance under certain well-known conditions, which need not here be repeated.

Outside the special case of complete similarity we do not know how the resistance varies with changes of scale along the principal axes, but it is clear that the formula for resistance must contain all the principal dimensions, and that they must occur in such a form that the law of similitude is satisfied for the special case of complete similarity.

Let l stand for any one of the principal dimensions and let $\frac{V}{\sqrt{l}} = c'$, then we are in accordance with the law of similitude if we write—

$$R_r = b f(L, B, D) \phi(c') \qquad (7)$$

provided

$$\frac{f(n L, n B, n D)}{f(L, B, D)} = n^3 \qquad (8)$$

where n is any positive figure, integral or fractional. Now, it appears that equation (8) can only be satisfied if f is an algebraical function of the form—

$$f(L, B, D) = \Sigma (k L^x B^y D^z),$$

where x, y, and z may be integers or fractions, positive, negative, or zero, but the sum of $x + y + z$ must equal 3, and k is a co-efficient, which may differ in the different terms. Thus f (L, B, D) might be the sum of a number of terms such as $k_1 B^2 D$, $k_2 B D^2$, $k_3 B^2 L$, &c. There may even be terms the exponents of which involve irrational quantities, provided always that the sum of the exponents is 3.

The principal dimension, l, occurring in c', is generally assumed to be the length L. The choice of L is probably suggested by the fact that it is necesary that $\frac{V}{\sqrt{L}}$ shall be the same in the ships compared, in order to obtain the same configuration of the waves relative to the ship, and hence the same wave interference, while no such condition appears to exist for the other dimensions. But, evidently, the law of similitude would be satisfied, if we write—

$$c' = \frac{V}{\sqrt{B}} \text{ or } \frac{V}{\sqrt{D}},$$

and, since in that case, also, $\frac{V}{\sqrt{L}}$ would be the same in similar ships, the wave interference would be similar. We know, on the other hand, from certain serial experiments, mentioned hereafter, that L must occur in the denominator of the expression for the wave resistance, since an increase in length scale is accompanied by a reduction in wave resistance, and it seems likely, therefore, that V and L occur together in the form $\frac{V}{\sqrt{L}} = c$, at least in the term expressing the effects of wave interference.

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Outside this term it appears that the value of L, although still occurring in the denominator, should, in ships with a cylindrical middle body, be modified. It was found by Mr. W. Froude that the insertion of a cylindrical middle body of increasing length in a certain model caused the residuary resistance at any given speed to fluctuate about a constant mean value. These mean resistances may on the diagram given by Mr. Froude* be represented by straight, level lines. Thus the mean residuary resistance was independent of the total length, and depended only on the length of entrance and run.

Hence the length dimension, occurring in the wave resistance term, should in ships with cylindrical middle body not represent the total length, but the total length minus the length of cylindrical body, *i.e.*, the sum of entrance and run. This length shall here be denoted by L_0.

In ships having a quasi-cylindrical middle body, as, for instance, most cargo steamers and many vessels of the battleship class, which have a plane bottom and plane sides down to the bilges for a certain length amidships, the same rule may be expected to apply, *i.e.*, a certain reduction should be made in their length in order to obtain the virtual length, L_0.

Without further experiments on other types of ships, it cannot be asserted that this rule is of general application, or that it takes account of all the effects of the cylindrical middle body. The discrepancy in the curves on Fig. 7 (Plate XXI.), as also in the co-efficients for the Merkara Model D, Fig. 1 (Plate XXV.), throw some doubt on the completeness of the rule. Since it is not, however, disproved by the results of the present analysis, it is here proposed as a first approximation.

Based on these and other considerations, related to the above remarks on the formation of ship waves, but which space does not permit to state in this paper the following expression for the total average resistance is suggested [see equation (4)].

$$R = f S V^{1\cdot825} + r \frac{B^2 D}{L} V^2 + p \frac{(B D)^{\frac{3}{2}}}{L^2} V^4 + q \frac{B^3}{L^2} V^6. \qquad (9)$$

As stated above, an analysis by which this formula might be established or perfected has not been attempted in the present paper. The three last terms have, as in equations (5) and (6), been replaced by one of the simpler terms $\beta_4 V^4$ and $\beta_6 V^6$ and β_4 and β_6 have tentatively been given several different values, which have been tested by means of the available serial experiments. The expressions which have given the most satisfactory results are—

$$\beta_4 = b_4 \frac{B^2 D}{L^2} \text{ and } \beta_6 = b_6 \frac{B^2 D}{L^3}.$$

The principal steps in the analysis and its main results are as follows:—

(*a*) *Changes in Transverse Scales.*—In Trans. Inst. N.A., 1904 (Fig. 6, Plate XXVI.), Mr. R. E. Froude has given a set of curves of power constants **C** for varying B and

* Trans. Inst. N.A., 1877, Plate VIII. (Fig. 3).

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D scale, which has been analysed, as explained in Appendix II., Section 3, for two alternatives, $R_r \propto T^2$ and $R_r \propto T^3$, where T stands for any one of the principal transverse dimensions, B or D.

A glance at the focal diagrams and the curves on Plate XXIII. (Figs. 8 to 18) shows that the relation $R_r \propto T^3$ gives the best result, giving indeed a perfect correspondence with the experimental results for all speeds between $c = \cdot 8$ and $c = 1 \cdot 35$.

This result is further corroborated by Colonel G. Rota's experiments* on changes of scale along each one of the principal axes. The model from which Colonel Rota derived his various forms was that of a cruiser, somewhat finer than the models of Mr. Froude. This investigation is described in Appendix II., Section 2, and the results appear on Plate XXII. (Figs. 1 to 10) and in Table I.

It appears that with changes in beam scale (models 5 to 8) both β_4 and β_6 vary as B^3, and for variations in draught scale (models 9 to 12) β_4 and β_6 vary as D, β_4 and β_6 giving about equally good correspondence. Thus, whether we use the formula for R_4 or R_6, we have $R_r \propto B^2 D$, which $\propto T^3$, as found by Froude's experiments.

Finally, Colonel Rota has investigated the effect of simultaneous changes in B and D scales, but with constant areas of cross sections, the ratio of $\frac{B}{D}$ being varied within very wide limits.† As explained in Appendix II., Section 2, and as seen from Plate XXII. (Fig. 11) these experiments show again that the resistance varies, at least roughly, as $B^2 D$.

The author has not had at his disposal any serial experiments on the expansion of beam and draught scale in ships of full form; but, judging from the consistency in the values of b_4 and b_6 in ships of all types, both full and fine, the formula $R_r \propto B^2 D$ appears to be of general application.

(*b*) *Changes in Length Scale.*—The following serial experiments have been analysed :—

(1) A set of power curves for models of the *Connecticut* type of constant cross sections. Each curve corresponds to a certain speed, and is plotted on lengths (displacements) as abscissæ. (Plate XXIV., Figs. 7 and 8.)

(2) Colonel Rota's experiments with varying length scale. (Plate XXII., Figs. 1 to 4.)

The result of these two series is that β_4 varies fairly as $\frac{1}{L^3}$, β_6 approximately as $\frac{1}{L^3}$, *i.e.*, in both cases as $\frac{n}{2}$. This is apparent from Table I., which shows the relative constancy of b_4 and b_6 for the two series. Also in Mr. R. E. Froude's series, type

* Bulletin de l'Association Technique Maritime, 1900.

† Trans. I.N.A., 1905, Plate LXXVIII. (Fig. 3).

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TABLE I.—*(continued)*. [*To face page* 214.

Ship or Model.	Extreme in W.L. Ft.	Mean Draught. Ft.	Wetted Surface. Sq. ft.	Cylindrical Co-efficient. δ	f_4	b_4	f_6	b_6	Froude's Co-eff. of Friction. f	Remarks.
Greyhound Ship	·3	18·65	16,544	·530	·0077	·522	·0096	·309	·0087	
Greyhound Model.. ..	·4	14·42	12,793	·530	·0084	·525	·0098	·344	·0087	
Merkara A	·2	12·03	10,674	·530	·0089	·533	·0099	·361	·0087	
,, B	·9	16·12	16,410	·530	·0085	·429	·0108	·269	·0087	
,, C	·1	10·40	10,600	·530	·0094	·453	·0103	·347	·0087	
,, D	·8	8·01	8,165	·530	·0096	·468	·0101	·345	·0087	
W. Froude, 1877, Cyl. Middle	·4	14·03	12,780	·583	·0083	·635	·0104	·350	·0087	
R. E. Froude, 1881, Ship A	·4	13·68	12,800	·589	·0096	·744	·0121	·372	·0087	
R. E. Froude, 1881, Ship A	·5	5·25	2,146	·553	·0086	·580	·0096	·337	·0091	For these Models the frictional term has been taken to vary as $V^{1·854}$
	·5	16·0	16,780*	·576	·0088	·595	·0100	·487	·0089	
	·1	11·07	8,061	·580	·0090	·584	·0106	·489	·0090	
Col. Rota's Model No. 1	·2	17·2	12,925	·587	·0079	·700	·0101	·551	·0089	
,, ,, 2	·17	·913	73·3*	..	·0097	·704	·0123	·438	·0103	
,, ,, 3	·11	1·31	97·2*	·653	·0090	·910	·0120	·490	·0103	
,, ,, 4	·11	1·31	97·2*	·661	·0097	1·060	·0136	·726	·0103	
,, ,, 5		..	..	·625	·0094	·881	·0114	·751	·0089	
,, ,, 6	·42	15·96	12,718	·649	·0086	·902	·0115	·824	·0089	
,, ,, 7	·1	13·13	10,763	·692	·0081	1·30	·0110	1·229	·0090	
,, ,, 8	·84	1·024	70·1*	·555	0	·500	·0016	·265	·0103	Residuary Resistance Curves.
,, ,, 9	·10	·798	59·2*	·585	0	·672	·0019	·485	·0103	
,, ,, 10	·44	1·206	72·9*	·595	0	·570	·0017	·463	·0103	
,, ,, 11	·20	·365	39·8*	·607	0	·827	·0014	·516	·0103	
,, ,, 12	·28	1·062	78·2*	·653	·0005	·375	·0010	·361	·0103	

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A and B, the b values are fairly constant for changes in length constant, **M**. (See Table I.)

The result obtained for changes in length scale corroborates that found for changes in transverse dimension scales. If, as found above, the transverse dimensions occur to the third power in the numerator of the expression for residuary resistance, the law of similitude requires, in fact, that the length shall occur in the denominator to the $\frac{n}{2}$ power, where n is the exponent of V. This condition is seen to be fulfilled. Summing up, it is found that, if we make $n=4$, the term β_4 of equation (5) is best expressed by $b_4 \frac{B^2 D}{L^2}$. Thus equation (5) may be written—

$$R_4 = f_4 S V^{1\cdot825} + b_4 \frac{B^2 D}{L^2} V^4. \qquad (10)$$

The Term Involving the Fineness.

The fineness is generally defined by several co-efficients, but we shall here make use only of one, the cylindrical co-efficient—

$$\delta = \frac{\text{Volume of displacement}}{\text{Length} \times \text{Area of midship section}},$$

which is generally assumed to have a very decisive influence on the resistance. In ships with a cylindrical middle body, this middle body has been neglected in the determination of δ, and the value so found is denoted by δ_c. This is a logical consequence of using L_0 instead of L.

It is clear that the cylindrical co-efficient can only in a very imperfect manner define the form, since a given value of δ may be obtained in different ways on given principal dimensions, depending on the distribution of volume both in a horizontal and vertical sense. But, while there are an infinite variety of forms, which will give a certain value of δ, and which may give very different resistances, it seems probable that there will be a certain type with a certain character of the lines, which, apart from wave interference, for a given value of δ, and given principal dimensions, will give what in practice may be considered average resistance, *i.e.*, an average value of b. Deviations from this average type take place in accordance with the service for which the ship is designed, and may cause great differences in the co-efficient b. Corresponding to the average type, we may, however, expect a relation, although perhaps somewhat vague, to exist between b and δ.

In order to discover this relation, values of b_4 and b_6 were plotted on values of δ as abscissæ, using all the available experiments analysed by the author. The curves, which represent the mean locus of the points so obtained, were found to be parabolic, and to be represented approximately by an equation of the form— .

$$b = s + t(\delta - \cdot 53)^2 \qquad (11)$$

On Plate XXV. (Fig. 1) the plots for b_4 are given, and a parabola conforming to

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equation (11) is drawn to represent mean values of b_4 for ordinary types of vessels. The equation to this curve is—

$$b_4 = \cdot 53 + 31 \cdot 0\ (\delta - \cdot 53)^2.$$

No scientific value is claimed for equation (11), but it seems to represent in a rough and general way the relation between fineness and residuary resistance, and it indicates that for ordinary forms of ships this resistance has a minimum when the cylindrical co-efficient has a value of about ·53. It does not follow that this co-efficient is the best in point of resistance, even for high-speed vessels; the skin friction and the effect of wave interference at the designed maximum speed may make it of advantage to adopt a different, generally a higher, cylindrical co-efficient.

As seen from Fig. 1 (Plate XXV.) some of the points fall very much below the average curve. This occurs particularly in models of full form, and is, at least in some cases, probably due to excessive breaking of the bow wave and pronounced eddy formation. Equation (11) can therefore be expected to hold good only in ships of ordinary forms and of not too great fulness.

It may not be without interest here to mention that an examination of a number of plane water lines and other ship-shaped curves has given the result, that the maximum obliquity, ϕ, of these lines relative to their line of symmetry, may be approximately represented by an expression of the same form as (11), viz. —

$$\sin \phi = s' + t' (\gamma - \gamma_0)^2,$$

where γ is the co-efficient of fineness of the curve, and γ_0, the value of which appears to be about ·55, is the value of γ for which the maximum obliquity is smallest.

Summary.

Referring to equation (1), it has been proposed to make $f_1(V) = V^4$. Both the R_4 curves and the R_6 curves show a fair correspondence with the experimental curves up to $\frac{V}{\sqrt{L}} = 1 \cdot 25$, but the exponent 4 has been chosen in preference to the exponent 6 on account of: (1) the better correspondence of the R_4 curves at very high speeds, and (2) the almost perfect and consistent correspondence of f_4 with the experimental co-efficient of friction as compared with the excessive value found for f_6 in most cases.

The exponent of V being settled, the form of f_2 (L, B, D) was determined by an analysis of serial experiments involving changes of scale along the principal axes, and it was found that approximately—

$$f_2(L, B, D) = \frac{B^2 D}{L^2},$$

which is consistent with the law of similitude.

Finally the form of $f_3(\delta) = b$ was studied by plotting the values of (b, δ) for all the

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experiments analysed, and it was found that this relation might be approximately expressed thus—

$$b = s + t\,(\delta - \cdot 53)^2,$$

where s and t, with a given cylindrical co-efficient, are dependent on the general character of the lines. Thus we arrive at the following final expression :—

$$R = f\,S\,V^{1\cdot825} + [s + t\,(\delta - \cdot 53)^2]\,\frac{B^2\,D}{L^2}\,V^4 \qquad (12)$$

where f is the co-efficient of friction given in Froude's table, s and t must be determined experimentally or by comparison with ships of similar type, although not necessarily of same fineness or of same size and proportions. The units are pounds, feet, square feet, and knots per hour. In ships with a cylindrical or quasi-cylindrical middle body, δ and L should be replaced by δ_0 and L_0, found by neglecting the cylindrical middle body.

This expression is proposed only as a rough approximation to the resistance, apart from wave interference, and only for ships of ordinary form. Undoubtedly, a more accurate and complete expression can be devised, when further systematic, serial experiments are available. A knowledge of such a formula, especially if it can be made to include the effects of wave interference, cannot fail to be of great practical importance. Unfortunately, the existing tanks have so far not been able to devote much time to purely scientific experiments owing to pressing work on special, concrete problems; although, in spite of this fact, several exceedingly valuable series of experiments have been carried out. The results obtained in ordinary current work, although generally of a scattered nature, might nevertheless acquire scientific value if they were published, but in most cases they are, for various reasons, kept confidential. Not until tanks are established exclusively for research work, such as the one recently proposed by Sir William White, will problems like the present one and many others equally important find their solution.

My thanks are due to Mr. R. E. Froude, Naval Constructor D. W. Taylor, U.S.N., and Colonel G. Rota, R.I.N., for the data and the experimental material which they have kindly placed at my disposal. Finally I wish to acknowledge my indebtedness to Professor James H. Cotterill, having had the great privilege of his instruction in the theory of resistance at the Royal Naval College.

APPENDIX I.

Analysis by Focal Diagrams.

Let the resistance of a ship or model be given by a curve plotted on speeds as abscissæ and resistances as ordinates. If we assume that the resistance can be expressed by two terms, as in the equation—

$$R = \alpha\,V^m + \beta\,V^n \qquad (13)$$

it is possible by means of focal diagrams to test which values of m and n will make (13) most closely represent the given curve. Choose the values of m and n that are thought most suitable, and from the curve of resistance find the resistances corresponding to a series of different speeds.

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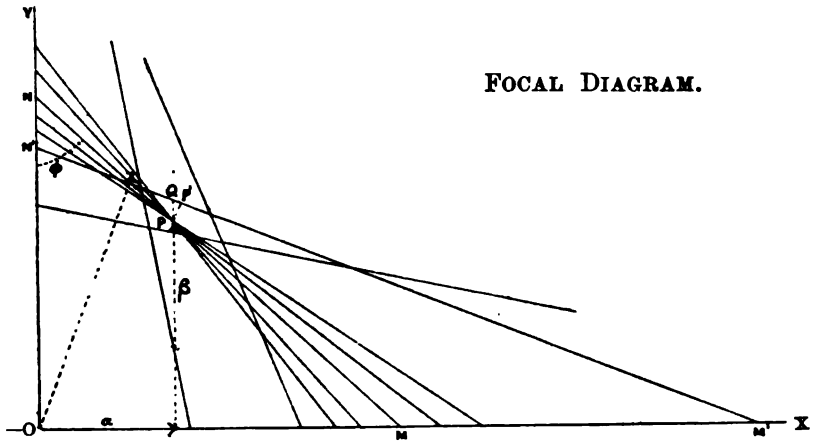
Each set of values of R and V will furnish an indeterminate equation in α and β of the form (13) If in such an equation we put $\alpha = 0$, we obtain a value for β, viz., $\beta_0 = \frac{R}{V^n}$, and similarly for $\beta = 0$ we obtain $\alpha_0 = \frac{R}{V^m}$. Mark off the values of α_0 and β_0 thus obtained from the different equations along the axes of a system of co-ordinates as on the accompanying figure, and join the points so obtained by straight lines ; then these lines will be the graphic representation of the equations.



FOCAL DIAGRAM.

If (13) represents the curve accurately, and if m and n have been rightly chosen, these lines will form a pencil, all meeting at the same point P, the ordinates of which, α and β, are the co-efficients in the equation to the curve.

If (13) does not represent the curve, the lines will not intersect in one point, and we must test other exponents of V by the construction of other focal diagrams, until the nearest possible approach to a perfect pencil is obtained. In practice, the curves of resistance do not conform to any such simple equation, because the total resistance, even apart from wave interference, cannot be accurately represented by two terms. All we can expect to represent by means of an equation of the form (13) is therefore an approximate average curve of resistance.

Hence we shall generally, if the curve extends to higher speeds, where the effects of wave interference are pronounced, find that some of the lines on the focal diagram fall below and some fall above the point of intersection of the lines corresponding to the mean curve. At very low speeds, *i.e.* at speeds less than 1 or 2 knots in case of models, errors of measurement will have a relatively great influence, and the lines corresponding to such speeds cannot, therefore, be considered reliable. It is, therefore, by the intersection of the lines corresponding to moderate speeds that we may expect to obtain the best guidance in the choice of the point of intersection, *i.e.*, in the choice of α and β.

Referring to the diagram, the direction of any line on the focal diagram is dependent only on the speed to which it corresponds, for let the angle formed by any line M N with O Y be ϕ, then $\tan \phi = \frac{\alpha_0}{\beta_0} = V^{m-n}$. Further, it will be found that the distance of any line from the origin is proportional to the resistance. If, then, a line, such as M′ N′, falls above the point of intersection P, it shows that the actual curve of resistance falls above the theoretical curve at the speed corresponding to this line, and conversely. Dropping normals from O and P on M′ N′ : O Z and P P′, the percentage fluctuation in the total resistance will be proportional to $\frac{P P'}{O Z}$.

The increment P Q on the ordinate through P represents the excess on β for a given value of α, and is proportional to the excess in wave resistance at the speed to which M′ N′ corresponds. It is thus easy by means of a focal diagram to estimate the percentage fluctuations in wave resistance, where the wave interference causes humps and hollows in the resistance curve.

Focal diagrams are, however, in certain cases deceptive, inasmuch as an equally good intersection may be obtained from the same curve, up to a certain speed, whether the formula : $R = \alpha_1 V^m + \beta_1 V^n$, or $R = \alpha_2 V^m + \beta_2 V^{n-m}$ is used, especially if m is small relative to n. Thus it is difficult, for instance, to determine by this method whether $R = \alpha_1 V^3 + \beta_1 V^6$, or $R = \alpha_2 V^3 + \beta_2 V^4$ is the correct solution.

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The value of a will, however, as explained in the paper, afford considerable guidance in this respect, and at very high speeds the difference between the two formulæ becomes apparent.

APPENDIX II.

Description of the Analysis.

1. *Early Experiments of Mr. W. Froude and Mr. R. E. Froude.* (Plate XXI.)

W. Froude.—*Greyhound.*—Ship and model. Trans. Inst. N.A., 1874. Focal diagrams and R curves are shown for the model on Figs. 1, 2, and 3 Plate (XXI.).

W. Froude.—*Merkara.*—Trans. Inst. N.A., 1876. Four models, A, B, C, and D, of same displacement but with different lengths of entrance and run, were tried. Models A and D had a cylindrical middle body. The focal diagrams and R curves for model A are given on Figs. 4, 5 and 6 (Plate XXI.).

None of the points for the *Merkara* models correspond well with the average curve (b_4, δ), Fig. 1, (Plate XXV.). The value of f_4 is small for the broad and deep model C, showing, apparently, great diverging waves, while, for model D, f_4 and f_6 are rather large, b_4 and b_6 rather small, pointing to great eddy resistance of some kind.

W. Froude.—*Insertion of Cylindrical Middle Body.* (Trans. Inst. N.A., 1877.) The parent model was that of a merchant vessel. Fore and aft body remained unaltered, and a cylindrical middle body of varying length was inserted. Referring to Plate VIII., Fig. 3, in the Transactions of 1877, the lines which can be drawn to represent the mean residuary resistance are seen to be straight, and nearly level, and on basis of the resistances corresponding to these lines, the focal diagrams (not shown) and the curves, Fig. 7 (Plate XXI.), have been constructed. The correspondence between the curves is not very good, a fact which is difficult to explain, since the effects of wave interference should in this case be completely eliminated. The experimental curve R shows a remarkably rapid rise, being parallel to the V^6 curve at about $c = \cdot 75$.

R. E. Froude, in his paper on the phenomena of the wave-making resistance of ships, in the Trans. Inst. N.A., 1881, gives several curves of residuary resistance, of which two curves for ship A have been analysed.

The focal diagrams and curves for 4,090 tons displacement are given on Figs. 8, 9, and 10 (Plate XXI.). While the V^6 curve is decidedly too high, the V^4 curve, as constructed, is probably somewhat too low. A falling off in the experimental curve after about $c = 1 \cdot 25$ is observed, which cannot be explained by wave interference alone. Since the experiment was not carried beyond $c = 2\frac{1}{4}$, we do not know whether the curve will again rise to greater steepness, and perhaps become parallel to the V^4 curve, as is the case with certain destroyer models.

2. *Colonel Rota's experiments.* (Plate XXII.)

Experiments on change of scale along each of the principal axes.—Bulletin de l'Association Technique Maritime, 1900.

The parent form is of the cruiser type, and the expansion along each of the principal axes comprises four stages. Models Nos. 1 to 4 give variations in length scale, Nos. 5 to 8 variations in beam scale, and Nos. 9 to 12 variations in draught scale. Models Nos. 2, 6, and 10 are identical, representing the parent form.

The points on the focal diagrams have been chosen so as to make the co-efficients β_4 and β proportional respectively to the expressions $\frac{B^2 D}{L^2}$ and $\frac{B^3 D}{L^3}$, whereby the values of b_4 and b_6 become constant for all changes in scale. Thus, for models Nos. 1 to 4 the products $\beta_4 L^2$ and $\beta_6 L^3$ are constant, for models 5 to 8 the ratios $\frac{B^2}{\beta_4}$ and $\frac{B^2}{\beta_6}$ are constant, and for models Nos. 9 to 12 the ratios $\frac{D}{\beta_4}$ and $\frac{D}{\beta_6}$ are

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constant. The curves R_4 and R_6 resulting from this choice of co-efficients are given with the curves R on Figs. 1 to 10 on Plate XXII., and it will be observed that, on the whole, R_4 and R_6 show a fair and about equally good correspondence with R.

Simultaneous changes in breadth and draught scale in models of constant length and constant area of cross sections. (Trans. Inst. N.A., 1905.)

These experiments have been analysed for values of $\frac{B}{D}$ ranging from 2 to 6 by means of the **C** values given on Fig. 3 (Plate LXXVIII.) of the 1905 Transactions, for different values of the speed constant **K**.*

The formula—

$$R_4 = f_4 \, S \, V^2 + b_4 \frac{B^2 \, D}{L^2} \, V^4$$

may be transformed to

$$\mathbf{C}_4 = k_4 + \mu_4 \, \mathbf{K}^2,$$

and

$$R_6 = f_6 \, S \, V^2 + b_6 \frac{B^2 \, D}{L^3} V^6$$

may be written

$$\mathbf{C}_6 = k_6 + \mu_6 \, \mathbf{K}^4,$$

where k_4 k_6 and μ_4, μ_6 are the same in similar models.

The values of **C** and **K** have been taken from Colonel Rota's curves, and by means of focal diagrams the constants, k and μ, corresponding to each of five different values of $\frac{B}{D}$ have been determined. On the basis of these constants, theoretical **C** curves have been calculated, and are, together with the experimental curves, given on Fig. 11 (Plate XXII.) denoted respectively by $\mathbf{C}_4$, $\mathbf{C}_6$, and **C**. It is seen that for values of **K** up to 2·5, the correspondence between the $\mathbf{C}_4$ and the **C** curves is very close for the whole range of the ratio $\frac{B}{D}$. In case of $\mathbf{C}_6$ the correspondence is not quite so perfect as for $\mathbf{C}_4$. Bearing in mind that b_4, b_6, and L are constant for all these models, which are of same fineness and length, and that, moreover, the product B D is constant, we should, according to the formulæ for R_4 and R_6 for any given value of **K**, have $R_r \propto \mu \propto B^2 \, D \propto \left(\frac{B}{D}\right)^{\frac{1}{3}}$ both for μ_4 and μ_6. If, then, it can be shown that $\frac{\mu}{\left(\frac{B}{D}\right)^{\frac{1}{3}}}$ is constant for all values of $\frac{B}{D}$, it is therewith proved that the residuary resistance is proportional to $B^2 \, D$. As seen from the accompanying table, this condition is only approximately fulfilled :—

$\frac{B}{D}$	μ_4	μ_6	$\frac{\mu_4}{\left(\frac{B}{D}\right)^{\frac{1}{3}}}$	$\frac{\mu_6}{\left(\frac{B}{D}\right)^{\frac{1}{3}}}$
2	·0115	1·35	·0082	·96
3	·0168	2·00	·0097	1·16
4	·0225	2·47	·0112	1·24
5	·0240	2·77	·0107	1·24
6	·0257	3·04	·0105	1·25

* The symbols **C** **K**, as also **M** and **L** are those used by Mr. R. E. Froude in his Constant-System of Notation (Trans. Inst. N.A., 1888), they are denoted on the Plates by enclosing the letters in a circle.

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At ordinary ratios of $\frac{B}{D}$ the co-efficient μ, and therefore the resistance, seems to increase somewhat faster than $B^2 D$ for increases in $\frac{B}{D}$.

3. *Later Experiments of Mr. R. E. Froude.* (Plate XXIII.)

These experiments were published in the Trans. Inst. N.A., 1904.

The parent form was that of a cruiser, and, as presented in Mr. Froude's paper, this form was varied by simultaneous changes in beam and draught scales, the length and the ratio $\frac{B}{D}$ remaining unaltered. Two ratios $\frac{B}{D}$ were tried: Series A, for which $\frac{B}{D} = 2{\cdot}6$; series B, for which $\frac{B}{D} = 3{\cdot}5$. The parent form was further varied by snubbing of the ends, whereby altogether six types were produced for each of the series A and B.

From the tables of **C** values given in Mr. Froude's paper, the resistances have been calculated for a ship of 300 ft. length; in case of the A series for **M** = 5·9, 7·0, and 7·9; in case of the B series for **M** = 5·9, 7·9, and 9·4. The range of speed has been extended as far as the tables permit, *i.e.*, from about—

$$\frac{V}{\sqrt{L}} = {\cdot}6 \text{ to } \frac{V}{\sqrt{L}} = 1{\cdot}5.$$

Based on these resistances and speeds, focal diagrams and curves for R, R_4, and R_6 have been constructed, the curves being shown on Figs. 1 to 5 (Plate XXIII.).

The co-efficient of friction f_4 shows here, as in most other cases (see Table I.), a very good correspondence with the experimental co-efficient, f, while f_6 is considerably larger. If the residuary resistance is proportional to $B^2 D$, we should expect to find b_4 and b_6 constant for the different **M** values; as seen from Table I., this is approximately the case. A more definite and conclusive test of the variation in resistance with changes in transverse dimension scales is obtained by an analysis of the curves given by Mr. Froude on Fig. 6 (Plate VI.) of the 1904 Transactions. This diagram gives **C** values to base of displacement, each curve corresponding to a certain length-speed constant; but since the length is constant (350 ft.), each curve will give **C** values for a certain speed. Also, since the cross-sectional areas increase directly as the displacement, and $\frac{B}{D}$ is constant, we have the displacement $\Delta \propto T^2$, where T represents one of the transverse principal dimensions. If, now, we assume the residuary resistance R_r to be proportional to T^2 $\left(\text{as, for instance: } R_r = b \frac{B D}{L^2} V^6\right)$, we shall have $C_2 = A_2 \Delta^{-\frac{1}{3}} + B_2 \Delta^{\frac{1}{3}}$; while, if we assume $R_r \propto T^3$ $\left(\text{as, for instance: } R_r = b \frac{B^2 D}{L^2} V^4\right)$, we obtain $C_3 = A_3 \Delta^{-\frac{1}{3}} + B_3 \Delta^{\frac{1}{3}}$. The A^2 and B^2 are constants, to be determined by focal diagrams. The focal diagrams and curves constructed on basis of these formulæ are given on Figs. 8 to 18 (Plate XXIII.). The excellent result obtained by the formula, which makes $R_r \propto T^3$, seems to prove that this formula is correct for this type of model, and up to speeds such that **L** = 1·35.

For the types A 3 and A 6 curves have been constructed for **M** = 7·0 (Plate XXIII., Figs. 6 and 7). Comparing with A1, **M** = 7·0, this investigation gives b values for three related types, with different cylindrical co-efficients.

4. *Naval Constructor D. W. Taylor's Experiments.* (Plate XXIV.).

(*a*) *Isolated experiments on a number of models.* The results of this analysis are given in Table I., and curves for some of the models are given on Plate XXIV. (Figs. 1 to 6). As regards the relatively low value of b_4 for Model ϵ (Fig. 1, Plate XXV.) no explanation can be offered, the lines not being known to the author.

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(*b*) *Experiments with a series of six models of* 1,000 *lbs. displacement*, same length and same ratio $\frac{B}{D} = 2{\cdot}923$, also with same midship-section co-efficient, ·926, but with varying cylindrical co-efficient, δ, which ranges from ·48 to ·68. On Plate XXIV. (Figs. 11 to 13) the R curves for three of these models are given. For $\delta = {\cdot}48$ the R_6 curve gives a better result than the R_4 curve.

The six spots for (b_4, δ), plotted on Fig. 1 (Plate XXV.) show a fair agreement with the mean parabolic curve. The value of f_4 is for these models practically zero, as might be expected, since the curves are for residuary resistance. Also the f_6 values are here very small.

(*c*) *A series of experiments on five models of varying fulness and displacement*, but of same midship section and same principal dimensions, approximately those of the *King Edward* class. (Proc. U.S. Naval Institute, March, 1907.) The value of δ ranges from ·618 to ·698. A propulsive efficiency of ·55 has been assumed in deducing the resistance from the I.H.P. curves given in the Proceedings. The values of (b_4, δ), plotted on Plate XXV. (Fig. 1) are seen to fall somewhat below the curve for average values, especially for the models of greatest fulness. Possibly these latter models have a nearly cylindrical middle body for part of their length, which would somewhat modify the result. The low values of f_4 point to pronounced formation of diverging waves.

(*d*) *A series of experiments on models of varying length scale, but constant cross sections.* The parent form is very similar to that of the United States battleship *Connecticut.* The curves from which the data are taken are referred to as "iso-speed" curves; they give power on lengths (displacement) as abscissæ. (Trans. Soc. N.A. and M.E., 1907.) The resistances are derived from the iso-speed curves corresponding to five different displacements, ranging from 20,000 tons to 29,000 tons; for each of which focal diagrams and R curves have been constructed. The R curves for the two extreme displacements are given on Plate XXIV. (Figs. 7 and 8). The constancy of b_4 at the different displacements, as seen from Table I., indicates that the resistance varies very approximately as $\frac{1}{L^2}$.

(*e*) *A series of experiments on models of destroyers of same displacement, length, and beam, but varying draught and co-efficients of fineness.* The value of δ was varied between the limits ·44 and ·75. (Thesis of Assist. Nav. Constructor I. I. Yates, U.S.N., of the Graduating Class, 1907, Mass. Institute of Technology). These experiments are of particular interest, because they were carried to very high speeds, up to $c = 2{\cdot}9$.

Fig. 9 (Plate XXIV.) shows the curves for a destroyer, C, of medium fulness, and Fig. 10 the curves for a destroyer, F, of unusual fulness ($\delta = {\cdot}75$) combined with a small midship sectional area and small draught. For this latter model b_4 and b_6 are very small, but f_4 and f_6 unusually large, pointing to the existence of a pronounced breaker at the bow and possibly also eddy formation at the stern. The conditions seem, in fact, to approach those which obtain by the motion of a log or prism through the water, where the eddy resistance predominates over the wave resistance.

In groups *a* and *d* the exponent 1·854 has been used instead of 1·825 in the frictional term of the expression for R_4 and R_6. This does not, however, influence the value of *b* appreciably, and the values of f_4 and f_6 relative to each other remain practically the same.

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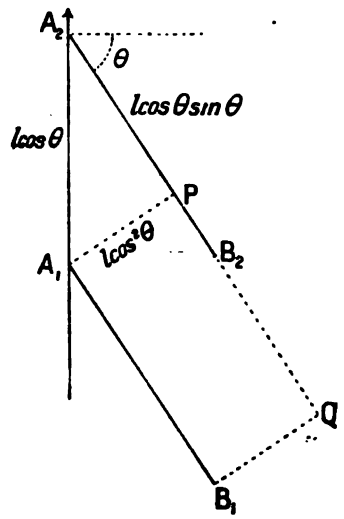
DISCUSSION.

Written Contributions to the Discussion.

Mr. R. E. Froude, F.R.S., LL.D. (Honorary Vice-President): I regret that I have not as yet had leisure to study this paper as carefully as I am persuaded that it deserves. It appears to me to be a well-conceived and suggestive contribution to a very important study—namely, the systematic analysis of empirical data concerning hull resistance, for the purpose of harmonising and correlating them, and so extending their conclusions to combinations of hull form other than those actually subjected to experiment. I may be misunderstanding the author's meaning, but as I read the section headed "The Speed Term," it seems to ignore the principle that the wave system extends itself sternwards from the hull (and in other directions to correspond), not at the speed of progress through the water, but at half that speed. Professor Hovgaard can, perhaps, clear this point up in his reply. This point is the only one which struck me off hand as questionable in respect of principle; but in a study of this sort there are sure to be many points debatable in respect either of convenience of procedure or of relevance to practice, and therefore I hope that all who have opportunity and qualifications for the task will give careful and critical attention to the paper.

Professor W. Hovgaard (Member): If I understand Mr. Froude's criticism rightly, he has chiefly in mind the expression given in the paper for the resistance connected with diverging waves, since that relating to transverse waves does not differ from the expression generally accepted ($R = \frac{1}{16} w \lambda_0 h^2 \propto \lambda_0 h^2$, where w is the weight of 1 cubic ft. of water). As to the diverging waves, let us imagine that we are looking at the bow wave from forward; we see that it has its greatest height near the stem of the ship, and that it is here of great steepness, the appearance being generally that of a crest in the act of breaking. If we fix our attention upon a particular point of the wave form near the stem, and follow its changes, we may observe this breaking in progress until it is completed, and until the wave has subsided to a negligible height. In doing so, our eye will have passed steadily outwards along the crest until it has arrived at the outer end of the wave. Thus the bow breaker presents at any instant a picture of all the successive stages of a wave in the act of breaking, and we might conceive it as a solidified mass moving parallel to itself in the same direction and with the same velocity as the ship, were it not that the actual motion of the particles and the sound produced force us to realise the action that takes place. In fact, the wave is always rolling on in a direction normal to its crest line, with a velocity V cos θ relative to still water, while relative to the ship it slides outwards in the direction of its crest with a velocity V sin θ, at the same time diminishing in height.



Thus we might expect that a gap would be left at the inner end, but as we know that the configuration of the wave relative to the ship remains unaltered, it is concluded that a process of renewal takes place by a constant addition of new wave of maximum height to the inner end. This addition takes place at the rate V sin θ. Consider the diagram shown here. At a certain instant let A_1 B_1 be the initial oblique wave (the bow breaker). At the end of a time equal to its natural period it will have travelled a distance equal to its own length, $l \cos^2\theta$, and will have arrived at the position PQ, but in so doing it will generally have lost the greater part of its energy in breaking. We may probably suppose that half the remaining energy is left behind in A_1 B_1, forming half the energy of the first of the diverging waves proper, while the other half of the energy of this diverging wave is supplied by the wave next behind. Thus, by the time A_1 B_1 reaches PQ, it contains the energy of

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only half that of a diverging wave, and the outer end B_2Q is so low that it may be considered negligible. Meanwhile, the point A_1 of the ship has advanced to A_2, the distance $A_1 A_2 = l \cos\theta$, and a new piece of wave, $A_2 P = l \cos\theta \sin\theta$, has been formed by the ship, so that as a final result the wave $A_2 B_2$ is an exact reproduction of $A_1 B_1$. Let h_0 be the height of the bow wave at the end nearest the ship, after making a deduction for the height of the transverse upheaval of the underlying deeper layers, then the energy supplied by the ship per second would be $\frac{1}{8} w h_0^2 l \cos^2\theta \times V \sin\theta$, if the wave is assumed to be a deep-sea oscillating wave. This assumption is not strictly correct, but probably we may assume the expression to be of the form stated. Since, moreover, on account of breaking, only a fraction of the energy becomes available for the creation of diverging waves, we shall multiply the expression for the energy by a factor k, smaller than 1. Thus the resistance corresponding to the diverging waves may, for one side of the ship, be expressed as follows: $R = \frac{1}{8} k w h_0^2 l \cos^2\theta \sin\theta \propto h_0^2 l \cos^2\theta \sin\theta$, as given in the paper.

Mr. Froude's treatment of the same problem in his paper on wave-making resistance, read before this Institution in 1881, is different. He measures the energy which lags behind, while I have attempted to measure the energy supplied, but it appears to me that there is no conflict between the results. Mr. Froude has shown that for each time the ship travels through the distance $2 l \cos\theta$, one diverging wave lags behind, and hence, denoting by E the energy of one diverging wave, he gives the following expression for the resistance—

$$R = \frac{E}{2 l \cos\theta}$$

According to the theory here proposed, half the energy of a diverging wave is left behind in $A_1 B_1$, while the ship travels from A_1 to A_2, which so far corresponds with the conclusion of Mr. Froude. Let λ be the length of the diverging waves along their crest, and let h be the equivalent uniform height—*i.e.*, that height which, if uniform throughout the length λ, would give the same energy of the wave; then, from Mr. Froude's formula—

$$R = \frac{\frac{1}{8} w h^2 \lambda l \cos^2\theta}{2 l \cos\theta} = \frac{1}{16} w h^2 \lambda \cos\theta.$$

Equating this expression with that obtained above, we have: $\frac{1}{16} w h^2 \lambda \cos\theta = \frac{1}{8} k w h_0^2 l \cos^2\theta \sin\theta$, or

$$h^2 \lambda = 2 k h_0^2 l \cos\theta \sin\theta.$$

This would show that the smaller θ is, the shorter would be the diverging waves, and in the limit when $\theta = 0$ we should have $\lambda = 0$, and therefore $E = 0$, *i.e.*, there would be no diverging waves proper. I doubt, however, whether it is physically possible even to approach this condition, and I have not as yet observed θ less than 49° in any ship.

It is of interest to examine what k would be under certain conditions. In the S.S. *Creole* (Southern Pacific R.R. Company), going at 16 knots, I estimated the maximum total height of bow wave to be about 6 ft., and, allowing for the underlying upheaval, it was estimated that $h_0 = 4$ ft. This is believed to be a minimum estimate, for the maximum height of the following diverging waves was also estimated at 4 ft. The angle θ was observed with great care and found to be about 60°. The length, normal to crest, of a deep-sea wave at 16 knots is $l = 142$ ft., and the length of the diverging waves along the crest was estimated $\lambda = 100$ ft. Assuming the equivalent uniform height of the diverging waves $h = 2$ ft., we get by substitution in the above equation:—$400 = 2k \times 16 \times 142 \times \frac{1}{2} \times .866$, or $k =$ about $\frac{1}{5}$. Thus it appears that by far the greater amount of energy supplied to the bow wave is consumed in breaking.

I had not intended to present so detailed a discussion on this difficult subject until I had had an opportunity of more complete study and observation, but, the question being raised by Mr. Froude, I thought it best to explain my conception of the problem, however incomplete it may be. I feel, however, considerable diffidence in doing so, for until we know more about the actual configuration of ship-waves in different types of ships and at different speeds, it is impossible to go beyond the uncertain ground of theoretical speculations.

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A NEW METHOD OF RESEARCH WORK ON FLUID RESISTANCE AND SHIP PROPULSION.

By Herr H. WELLENKAMP, Member.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 10, 1908; the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D., President, in the Chair.]

BEFORE entering upon the real subject of this paper, I should like to refer to the principal points of the paper I read in Berlin in November, 1907, before the Schiffbautechnische Gesellschaft, as I presume that the method I am about to describe is unfamiliar to most of you. On that occasion I described the principles of a new method of towing ship models, and gave some results of experiments in the Imperial Dockyard at Kiel. The subject that is to be brought to your notice to-day concerns another step forward in this method of testing ship models. The towing of ship models alone does not suffice, as is well known, to solve the problems which confront shipbuilders. What we undoubtedly need much more urgently is a trustworthy, simple, and speedy method of investigating the effect of screw propulsion on models, and it is on this account especially that the new method will be found advantageous.

I cannot help remarking here that it was Sir William White who first induced me to deal with experimental tank problems by his valuable paper of 1904 on "The Establishment of an Experimental Tank for Research Work on Fluid Resistance and Ship Propulsion."* The arguments in favour of a tank specially devoted to such purposes could not have been presented in a more conclusive manner than was done in that paper, and in the discussion that followed it. It was there stated, and fully admitted, that a demand exists for a ready means of carrying on investigations concerning towing resistance, and of extending our knowledge of frictional and air resistance, and especially of improving upon the hitherto unsatisfactory experience with regard to screw propulsion, which is liable to inaccuracy arising from the smallness of scale. Although at the time of Sir William White's paper there was, of course, a general belief in the merits of the system, which we owe to the genius and the mechanical skill of the late Wm. Froude and his son Mr. R. E. Froude, there seemed to be little hope of obtaining information of practical value as to the quantitative results of experiments on screw propulsion.

In endeavouring to contribute as much as possible to the solution of the problem,

* Trans. I.N.A., Vol. XLVI. p. 39.

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and in order to meet these requirements, I have devised a method of overcoming at the same time the financial difficulties of tank experiments by introducing greater simplicity and enabling a shorter tank to be used; and also of avoiding the various difficulties arising from the use of delicate mechanism, which is said to be not always quite reliable and trustworthy. It was an almost imperative necessity to do away with the carriage, the rails, and the mechanical tractive power, and to employ a system of motion that would be purely self-acting, self-regulating, and self-registering. My method is, within certain limits, identical with earlier methods, especially with that which Beaufoy so effectually applied in the Greenland Docks at the end of the eighteenth century. When I recently became acquainted with Beaufoy's experiments, I was very much surprised to find how near he was to a complete solution of the problem. Nevertheless, it must be admitted that his system was in embryo, and it is remarkable that he was able to obtain any results at all. Certain it is, at any rate, having regard to the great difficulties arising from his system of motion and measurement, that Froude's towing method would never have been adopted if Beaufoy's system had not failed.

The chief consideration in dealing with towing experiments is that models should follow a direct course straight forward, totally free from deviations and disturbances, with a uniform motion and, as far as possible, with a constant velocity. This uniform motion is very difficult to attain in practice. For instance, a carriage on absolutely even rails, and fitted with the best type of wheels and bearings, will move at a speed which, perhaps, appears quite uniform, but the finest measuring instruments will show that there really are disturbances due to irregularities in the working mechanism employed, or to differences between towing force and resistance.

There are, however, to all intents and purposes, perfectly regular motions produced by nature alone, in which man has no hand, such as, for instance, the motion of a raindrop falling freely through still air, or of a body sinking in water, assuming that the shape of the body is suited to this motion.

It is obvious that the resistance which a ship or a model meets with when gliding through the water at a constant speed is perfectly constant also. Therefore it depends only on the kind of tractive power whether we attain a thoroughly uniform motion, as desired, or not. Nature herself gives us such a tractive power in the force of gravity. If we apply this force and take away all gear we may be sure that the motion becomes as uniform as possible. Nevertheless, there are still some very small irregularities such as occur throughout nature, arising from the minute oscillations due to friction and eddy resistance. But it is impossible to measure these irregularities, which are scarcely perceptible by the finest vibrometers, and do not interfere at all with the results.

Further, if we apply a tractive power which is not only quite constant, but also exactly equal to the resistance of the model at the actual speed, we obtain not only a perfectly regular motion, but also an exactly constant velocity. This is the object

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which we are striving to attain. All attempts hitherto made with apparatus utilising gravity as the tractive force have, as far as I am aware, been without satisfactory result, and it is doubtful whether the data obtained from previous experiments can be considered absolutely reliable.

I will now proceed to give a description of the towing arrangements which are shown in Fig. 1 (Plate XXVI.).

A very thin piano wire is attached to the end of the long bowsprit of the model, and is stretched horizontally above the water to the end of the tank, where it passes over a drum into a deep well. This drum has extremely fine agate-pointed bearings, and a polished surface which may be blackened with soot from a turpentine flame. The circumference of the drum is exactly 1 metre, and equal to one turn of the wire. The groove for the wire is on one side of the drum; and on the other side a scale is engraved. (Fig. 2.)

A towing weight hangs at the end of the wire in the well. This weight is rather small, because the resistance of a model at the highest speed attained is generally less than 1 per cent. of the weight of the model. A time recorder consisting of a big tuning fork, records by means of a needle point, one-hundredth parts of a second in the form of a wavy line on the revolving surface of the drum. To prevent the overlapping of the wavy line, a longitudinal movement is imparted to the needle point.

That constitutes the whole apparatus employed during the measuring run.

Let us now suppose that an experiment is to be made on a model under the tractive force of the towing weight (p_1) alone. At starting, the whole power of the weight is employed in producing kinetic energy and imparting it to the mass of the model, the wire, the drum, and the weight. As soon as any velocity is attained, resistance of the water is produced, and must be overcome by a corresponding portion of the power of the weight, thus leaving the remainder of the power available for further acceleration. This remainder will continually diminish and will finally disappear altogether, while the resistance increases correspondingly and becomes at last just equal to the whole of the towing weight, leaving nothing for further acceleration. From this moment the starting run, *i.e.*, the period of acceleration, is finished, and the measuring run, *i.e.*, the period of dynamical equilibrium, now begins. In order to realise what a very long tank would be necessary to attain dynamical equilibrium, if using only the towing weight, it must be remembered that even if resistance did not absorb power at all, and the whole of the towing weight were to produce energy, there would be required a starting run of at least 50 metres in length, for models of usual size and speed. But if resistance comes into play, this run must be four or five times as long before the velocity can become approximately uniform.

There is a simple way of reducing the starting run down to a few metres by giving the model an initial impulse by means of an additional starting weight,

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which is considerably heavier than the towing weight. This weight takes the form of a chain (P, in Fig. 1), and its action decreases from the full extent of its weight down to zero at the end of the starting run. It is temporarily connected to the model by means of a second special wire, so that various predetermined amounts of power may be transmitted. This wire takes hold of the fore end of the bowsprit by a clutch, which is released automatically as soon as the starting run is finished. The model then continues on its way under the influence of the towing weight only, and the measurements can then be made.

Whether or not a really constant velocity is attained must be determined afterwards by the help of the above-mentioned wavy line traced by the tuning fork. Since the wavy line takes the form of a spiral on the drum surface, owing to the longitudinal movement of the needle point, the whole measuring run is represented for every hundredth part of a second to full size. This enables us afterwards to ascertain whether the velocity has been constant or not, and to measure its amount. Should it be found that there has been acceleration or retardation $\pm \gamma$, it shows that the towing weight was either too large or too small. It follows that the (mass of the model $\times \gamma$) is the weight necessary for correction. After the towing weight has been altered by adding to or subtracting from it, the experiment must be repeated, and we shall then obtain a run with a uniform, or substantially uniform, speed due to the adjusted towing weight.

It must be remarked here that the heavy starting weight has to be accurately adjusted in order that the same energy may be transmitted to the mass when repeating the experiment, otherwise it would be impossible to attain the same velocity as before, and the repetition of the run would be a failure. If the second run does not yield a sufficiently constant speed, there is no difficulty in again determining the correction for the towing weight, and then making a third run, and so on. It will be seen that the method is thoroughly adapted to the most delicate scientific research work, by enabling us to advance gradually to the highest degree of uniformity o motion.

This result is attained, firstly, by means of the exclusive use of gravity as the most uniform towing power available; secondly, by doing away with nearly all apparatus, with its resulting disturbances; and thirdly, by adjusting the towing weight to correspond with the actual speed by trial and error. The consequence is real dynamical equilibrium, such as is required for an exact investigation.

After each run the tractive power and the velocity must be measured. The former is accurately known up to $\frac{1}{1000}$ kg., for it consists of the towing weight and the weight of the connecting wire. This is kept constant during the whole run by another supplementary wire or thread of double the linear weight, hanging from the upper part of the well in a bight to the lower end of the towing weight.

The velocity of the model is recorded on the drum surface, where the waves are given for each hundredth part of a second to full scale. It is of great value for

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accuracy of measurement that neither the curve of time nor the curve of velocities has to be drawn. We find the velocities clearly shown by cross curves passing through adjacent waves. In order to attain greater accuracy the wavy line is divided by a straight spiral line, and the crossing points of the two can be exactly determined.

Whether an experiment made as above indicated has been successful or not, *i.e.*, whether the result has been disturbed or not by any outside influence, is seen by reference to the cross curve on the drum. This curve must be of a smooth character, free from irregularities.

If the cross curves appear as straight lines, then it follows that the velocity has been constant, but if they are concave or convex, then it follows that the velocity has been accelerated or retarded. The slightest amount of such curvature is easily perceptible, and capable of measurement.

Another advantage is that when the curve of resistance of a model for various speeds has to be drawn, it is no longer necessary to have a great number of points, as is the present custom: a fifth or fourth part of the present number of runs is quite sufficient, no point being found distant from the curve.

Although I have already said that it is generally necessary to make one or two preparatory experiments before dynamic equilibrium is reached, this is unnecessary if a set or series of experiments is made, beginning with low speeds and rising by small steps to high speeds. In this case each trial is a preparatory one for the next, and allows us to estimate almost accurately the increase of starting weight and towing weight for the following trial.

The method, therefore, enables us to obtain great accuracy of measurement, the production of speedy results, and a very great reduction of size of tank, without the necessity for a carriage or rails with their expensive foundations. Again, no engine gear is necessary, so that the initial cost, as well as the running expenses, are exceedingly small, and the management is very simple.

There are other advantages, some of which I will mention here. For instance, models on a larger scale can be employed than have been up till now, and the tank can be enlarged in breadth and depth without incurring great expense. Velocities may be increased without difficulty, far more than when using a heavy carriage with a large moment of inertia. Further, the process is open to the spectator, and is easy to follow, to control, and to photograph, all the more so because the apparatus need not be continuously watched or handled during the run, the results being automatically recorded, so that they can be inspected and measured when convenient. One single movement of a lever when starting the model is all that is required. It releases the model from the starting point, and makes the tuning fork sound either by the stroke of a hammer or by electro-magnetic contact. All the movements are made automatically—viz., the release of the starting weight from the model, the tracing of the needle of the tuning fork on the drum surface, and

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its sideways movement to obtain a spiral wavy line, and, finally, the braking of the model by a special heavy weight which is seized and raised at the right moment.

One of the principal advantages is that the method can be employed not only for towing models and testing resistances, but for all sorts of tank experiments. Before giving examples of other experimental work, I will discuss briefly certain objections that have been raised regarding the accuracy and the general applicability of the method.

It has been suggested that a motion of so short a period would not suffice to attain the dynamic equilibrium mentioned above, because the surrounding particles of water at some distance from the model are said to change their motion in a small degree after the model has attained uniform speed, so that these particles may alter the velocity of the model from behind to an appreciable extent; which means upsetting the equilibrium. But, although it is obvious that the motion of the model does not become constant until all the particles of water which can in any way influence the model have themselves reached a state of equilibrium, the question is, when does this moment arrive? Whether this state of affairs occurs or not, does not depend on any theoretical considerations, but is simply a statement of facts, depending entirely on the character of the cross curves. If they are really straight lines, there is no doubt that equilibrium has been reached. When they tried at Charlottenburg, by means of Woltmann's blades, to prove this suggested defect of too short a run and starting run, it was not observed that the apparent defects were attributable to the irregularities of the motion of the carriage and other defects.

I will now state as a result of my experience, and of the information obtained from diagrams made during the experiments at Kiel, that the lengths of the starting run and of the measuring run need not exceed $1\frac{1}{2}$ lengths of the model; the length of the tank ought to be $4\frac{1}{2}$ times the length of the longest model that is to be tested.

It has also been objected that the wire is attached to the bowsprit at too high a point, instead of at a point corresponding to the height of the propeller shafts. This is of no importance, as there is no difficulty whatever in eliminating this defect. We have only to consider that the towing power is accurately known, and the height of the wire above the propeller shafts as well; consequently, the trimming moment is also known. We therefore have only to trim the model before the trial begins, by displacing a small part of the weight of the model so as to counterbalance the trimming couple accurately, the model being then driven exactly as if by the screws.

Another objection raised is that the sagging of the wire causes a slight deviation of the towing force from the horizontal. To avoid this a small weight—it is really no more than $\frac{1}{1000}$th of the tension of the wire—may be removed from the end of the bowsprit, thus neutralising the small downward component of the towing force.

A further objection is as follows. Every ship is, while in motion, exposed to varying trimming forces resulting from the water pressure, which generally have a

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tendency to raise the bow (Fig. 3, Plate XXIV.). Thus the point of attachment of the wire at the end of the long bowsprit will be raised to a perceptible degree, and the direction of the towing wire will no longer be horizontal, and will therefore produce a defective trimming moment, which does not exist when the ship or the model is driven by screws. Since the amount of the raising of the bow cannot be foretold, it is necessary to observe this effect during the trial and afterwards to neutralise it before the next trial begins. We have only to observe the lifting of the point of attachment and to lower it by about the same amount. There is a sliding piece in a vertical slide-way fitted at the end of the bowsprit, which enables one to lift or to lower the point of attachment. Thus the horizontal direction of the towing force is secured, and the model is again under the influence of the towing force and the trimming moment, in exactly the same manner as if it were being driven by screws.

Screw Propulsion.

The chief consideration in the towing method was the dynamic equilibrium automatically produced under the influence of gravity alone. There is no reason to depart from this leading feature when dealing with research work on screw propulsion.

I will now give the chief features of the arrangement which I at first employed and afterwards improved upon for general application.

I had a model with the same bowsprit, wire, drum, and weight as already used in the towing experiments, and I attached another wire at the aft-end of the model, passing above the water surface over a pulley into another well at the aft-end of the tank, where a weight (p_a) equal to the forward one, is suspended. In this way the model is no longer towed by a weight, but is in perfect statical equilibrium. If by some other means the model is put in motion, its progress is recorded as before on the drum surface. The model may be fitted with one or more screws on propeller shafts just as in a ship, and may be driven by the action of the screws: *i.e.*, by thrust. The screws must be revolved by the most uniform force available, *i.e.*, by gravity. To obtain this we attach a third weight to the screw shaft by means of a very thin wire which is wound round the shaft as many times as there will be revolutions made during the whole run. The wire passes from the shaft vertically upwards over a guide pulley at the top of a mast on the model, and carries at its free end the third weight. This weight drives the model (not shown in the diagram) indirectly, by producing the force to revolve the screw, during its fall from the top of the mast.

The thrust produced depends upon the speed of rotation of the screw and upon the speed of the model. These both increase gradually while the model is exclusively under the influence of the falling weight. Assuming that there is a sufficiently long run, we may expect to reach at last a maximum number of revolutions, and consequently a corresponding maximum speed. The whole thrust then produced by the weight is absorbed in overcoming the total resistance of the model. This is not merely the towing resistance but also the "augment of resistance" (as Mr. Froude

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calls it) owing to the suction of the screw, sometimes called "thrust deduction." A condition of dynamic equilibrium having been attained, we may begin to measure: (1) the speed of the model, (2) the number of revolutions of the screw per second, (3) the work done by the sinking weight. There is still wanting only, fourthly, the thrust, which requires special apparatus for its measurement.

We have already seen that the moving force, or the thrust, acts on the model by means of the screw propeller in the same manner as in a ship. There is only one slight difference, viz., the shaft is not kept fixed by a thrust bearing, but passes through an almost frictionless ball bearing in the boss, which allows at the same time a longitudinal movement of the shaft within certain limits. The fore end of the shaft is sharpened to a point, which is hardened. This point presses against a cup-shaped agate-bearing placed in the short lever of a spring dynamometer, thereby moving it longitudinally to a small extent, say, $\frac{1}{8}$ in., or less, according to the magnitude of the thrust. By this small movement the thrust exerted can be measured, if another longer lever of the dynamometer is fitted with a pointer which records on a blackened surface. The spring dynamometer may be adapted to measure forces between 0 and 1 kg.; we then have to fix an additional counterbalancing weight to a counterlever in order to be able to also measure greater thrusts with the same spring within the same limits of shaft movement, and with the same accuracy.

It is evident that this apparatus cannot be employed when the model has to make a long run. The weight hanging down from the mast cannot exceed a definite amount because of the limited stability of the model. For practical purposes a longer run is required, and consequently the arrangement just described would not always suffice for making exact and trustworthy measurements.

To fulfil this requirement, another arrangement may be employed which is an improvement only in one respect, viz., by doing away with the third weight hanging down from the mast, and thus obtaining further simplification. The apparatus will then be as follows:—The first weight is, as we know, still attached to the wire at the bowsprit, and exerts a tension in the forward direction; the second weight is now used not merely to counterbalance the first one and to keep the model in statical equilibrium, but it is at the same time used to rotate the screw. It is therefore fastened to the shaft of the screw instead of to the aft end of the model. Its wire is wound round the shaft as was previously done, and passes over a guide pulley on the deck of the model and is led horizontally to the pulley placed at the well at the aft end of the tank. There it is kept stretched by a weight p_a of just the same magnitude as the corresponding weight p_f in the well at the fore end.

We now have the model, firstly in a state of statical equilibrium under the tension of the two wires fore and aft, which must both lie in one straight line to avoid a couple; secondly, under the moving influence of the aft weight, which is no longer fixed, as its wire can freely unwind, allowing it to fall in the well after being released. The screw shaft is thus revolved, and the model is driven forwards although the wire pulls

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backwards. This time the aft weight not only counterbalances, but is a moving one, and develops work.

Thus if

p_f = weight at fore end,
p_a = weight at aft end,
l = length of wire unwound during run,
l_f = distance traversed by the falling weight p_f,
l_a = distance traversed by the driving weight p_a,

then, the work done is $p_f \times l$, or $p_f (l_f + l_a)$, or, with reference to the equation of potential energies $(p_f \times l_f) + (p_a \times l_a)$.

I need hardly mention that the means described for preventing small defects in the towing experiments must be applied here as before. I refer to the means for eliminating the influence of the sagging of the horizontal wire, of its varying weight, of the trimming effect on the model, and chiefly to the use of a special starting weight to overcome the inertia of the mass of the model by producing virtual energy.

The measurement of the speed of the model is done in the manner already described by time-recording on the drum surface. The number of revolutions of the screw is recorded by a revolving disc moved at a slow rate by the guide pulley on the deck. On this disc, time in seconds is recorded by electrical means, and the thrust of the screw is recorded by the pointer of the dynamometer balance. This recording of time, thrust, and revolutions is done quite automatically. The two diagrams, the one on the drum surface, with hundredth parts of seconds, and the other on the disc with full seconds, can be combined very easily.

When starting experiments, we have to calculate as nearly as possible the probable value of the two suitable weights for the starting impulse and for driving the screw, and if the desired result is not attained, we have to change the weights in the same manner as described for the towing experiments, and thus any desired accuracy may be attained. But care must be taken that in this case the amount of the weight for driving the screw is attached both at the fore and also at the aft wire, to ensure exact statical equilibrium.

In this apparatus only three pulleys or drums and the shaft bearings are under stress. The friction of these has to be overcome, and that requires a special experiment to determine the amount of friction at different speeds and weights. Without such a test the trials would be defective. In order to ascertain the friction, we take off the blades of the screw and join the two fixed ends of the wires instead of fastening them to the shaft and to the bowsprit. This single wire is then led over the same drum and the same pulleys as in the trial, but somewhat differently arranged, as shown in Fig. 4 (Plate XXVI.), and the same weights are applied. The system is then put in motion by a small additional weight attached to one or other of the two weights at the ends of the wire. The speed attained is recorded, and the friction can be calculated. Thus the results of screw propulsion are as accurate as may be desired, and are ready for the complete investigation of all questions connected with this kind of experimental work.

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As mentioned above, there is no difficulty arising from the use of more than one propeller. Each of them may be driven by a special wire and weight, and each may also have its separate record of revolutions, thrust, and work. If desired, as will usually be the case, all wires can be kept at the same speed when led over one drum, instead of over separate pulleys, and any predetermined relative number of revolutions may be attained.

As to the experiments carried out up till now, I need only mention that there is a dry dock at the Imperial Dockyard at Kiel, fitted as a provisional experimental tank, where most of the interesting points of towing experiments have been investigated. The research work carried out on some models of new ships has shown the great efficiency and the trustworthiness of the method.

In consideration of its superiority and applicability to all kinds of experimental work, the new tank built by the Hamburg Government, as well as the new tank to be now built for the German Navy, are designed and will be installed on this new system.

I venture to hope that in this way a new field will be opened, as much for further practical information as also for the purpose of scientific progress in hydrodynamics.

DISCUSSION.

Mr. R. E. Froude, F.R.S., LL.D. (Honorary Vice-President): My Lord and Gentlemen, the interest that this paper excites in me is of a very pleasurable kind, not only because it describes a method of experimenting which seems to me in its details admirably conceived and carried out, but because it carries me back in memory more than forty years. I think it must have been in 1862 or 1863 that my father, the late Mr. William Froude, made a set of experiments upon models to which, so far as I know, he never made any public reference, although it was rather an elaborate set of experiments, and the apparatus he had for them was very carefully made, and, I need not say, was excellently adapted to its purpose. The interesting point for our present purposes is that the method of those experiments was, in all general features, word for word the same as has been described in this paper; and that is accounted for, I suppose, by the fact that they both had been independently modelled upon Beaufoy's method. Of course, my father's experiments were on a much smaller scale than what is contemplated here. He used for the purpose a large masonry rainwater tank that he had had built close to his house, but on one important point his plant was curiously identical with what is suggested here. So far as I can recollect, the length of his run was just about four and a half times the length of the model that is here mentioned. In one respect my father's apparatus perhaps had an advantage—that, working with such small models as he did, he was able to use an exceedingly effective impulse for imparting the speed, and to be content with a simple cushion buffer at the other end to stop the model, so that he was able to get the greater part of

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those four and a half lengths—in fact, I think he had very nearly four lengths—for what is called in this paper the "measuring run."

The results that my father got from these experiments were suggestive and important, and they certainly played an important part in the turn of events which led to the establishment of the Admiralty experimental works, inaugurating that system of experiment which has now been so very widely applied. But my father was certainly dissatisfied with the results, considered as quantitatively accurate, on the ground that in his opinion the run was too short, and he saw that if you gave a much longer run, there would be very serious trouble in the sagging of the towing lines, and that is the reason why, when the opportunity came to my father of designing an experimental tank for the Admiralty, he adopted the system which we now have. Now, the reason he concluded that the run was too short is very important in its bearing on this paper. He adopted just the same system as is recommended in this paper of giving a different series of impulses till he should find, as he hoped to find, that with the right impulse the model, when released, continued to travel at an absolutely steady speed; but what he found was, at the higher speeds, at any rate, that he could not realise this condition, because the model, although it might go at a steady speed for a short distance, perhaps a length at a comparatively steady speed, would then begin to accelerate again; and so he found that under certain conditions of experiment he could not get that steady speed which was necessary for accurate results. I remember perfectly well his being immensely perplexed at this. He could not see why it should be that when the model had reached its right speed, and was for the moment apparently steady at that speed, after a little while it should again accelerate under the same propulsive force. He afterwards found the explanation which we now know—viz., that in order that the hull should experience the resistance proper to permanent steady speed, it is not only necessary that it should be going steadily at that speed, but also that it should *have been* going at that speed long enough to constitute all the conditions of resistance which are proper to the permanent result. That is a matter which takes a considerable time, and particularly so as regards the, in one sense, most important element of all in resistance—namely, the wave-making resistance. We know that the wave-making actions of the different parts of the hull are not independent of one another, and that consequently, in order to have the result proper to ultimate steady speed, the influence of the wave making that is taking place at the bow must have penetrated back as far as the stern; and since the wave influence—as I have termed it—in some measure accompanies the wave system, and travels with it at half the speed of the system, it is in theory absolutely necessary for permanent conditions that the hull should have travelled at its steady speed for at least two lengths of the hull. Consequently, if we lay it down as is recommended in this paper, that the starting and measuring runs should consist of one and a half lengths each, then the experiment will be concluded half a length before it has any right to begin.

Now as against that, Herr Wellenkamp objects, and with great *prima-facie* reason, that the proof of the pudding is in the eating, that he does get steady speed, as shown by his records, and if so, what more do you want? Well, when we have before us a clear and certain theoretical principle such as I have mentioned, we require a very great deal of evidence to make us confident that it does not apply, and Herr Wellenkamp's contention raises a number of minor questions which I think would be too intricate to go into here. I must content myself with raising the question whether this speed-measuring apparatus, exceedingly delicate and well contrived as it certainly is, is accurate enough to detect with certainty the exceedingly small accelerations due to the resistance variations which are here in question, considering what an exceedingly small time there is for those accelerating forces to act. As regards that point, it is important to remember that what we are concerned with, really, during this one and a half lengths of measuring run, is not primarily variation in speed, but variation in resistance conditions; and variation in resistance does not directly mean variation in speed, but variation in change of speed—variation in acceleration. So the question is not really whether the speed at the end of this one and a half lengths is different from the speed at the beginning—that is a matter that you can set right by the impulse—but whether there was not during the first half of that travel—namely, the first three-quarters of a length, let us say—a retardation, and during the second three-quarters of a length an acceleration, or *vice versâ*; in other words, the length of

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travel on which we have to consider the acceleration acting will not be one and a half lengths, but only three-quarters of a length.

Now, just to see the sort of quantities we are dealing with, I have made a little calculation. I have taken what would be an average case in our experiments, of a model 14 ft. long and a speed of 7 ft. per second, consequently the three-quarters of a length would be travelled in 1½ seconds. The resistance at such a speed will be just about what is mentioned in the paper—about one-hundredth of the displacement. In our work we certainly expect to get our resistances within 1 per cent., and we are disappointed if we do not; so that we are concerned with such an acceleration as would be effected by a force of 1–10,000th of the displacement acting for 1½ seconds. Now, the speed produced by that is less than 1–200th of a foot per second, and the difference of time in one revolution of the drum (one metre circumference) due to that difference of speed, is only 1–3,000th of a second, and it seems to me that here we are really dealing with quantities altogether too small for satisfactory measurement.

Dr. GLAZEBROOK, LL.D., F.R.S.: My Lord and Gentlemen, I fear I have very little indeed to add to what has been so ably said by Mr. Froude. I am glad to have this opportunity of speaking here, and of recognising the great importance of the subject that has been put before us this morning in these two papers, and saying in public how much the Committee of the National Physical Laboratory appreciate the generosity with which Mr. Yarrow has come forward and offered, under certain conditions, to establish a tank at the Laboratory in order that experiments and observations of this very important character may go on there. Having that matter in view, Herr Wellenkamp's paper naturally attracted my attention, because it raises such very serious questions as to what the form, the dimensions, and the arrangement of the tank should be. I felt with Mr. Froude that any method that might be suggested for measuring the forces concerned and the speed of the model must be one of very great delicacy and very great ingenuity. I do not know whether Herr Wellenkamp got his ideas in the first instance from a very old piece of apparatus with which many of us have, I daresay, worked or have seen experiments made. I mean the old Attwood's machine, in which two equal weights are supported over a pulley and a smaller weight put on to produce acceleration for a certain distance, and then the resulting speed is measured. Those of us who have had cause to work with an Attwood's machine have realised the great importance of extreme delicacy in the apparatus used, and it certainly was brought to my mind that in order to secure results with the method that Herr Wellenkamp has described it was necessary that the apparatus should be of a very high order of efficiency. That point has been so ably brought out in what Mr. Froude has just put before us that I do not think I need endeavour to labour it any more. For myself, I should have been glad had it been possible to have had rather further information than is actually given in the paper as to the sizes concerned. We are told that the tank is about four and a half times the length of the model, and I think it may be inferred that the models were comparable in size with those used in ordinary tanks, but I am not sure about that point. If they were some 14 ft. long, it would mean, I suppose, that the tank need only be 50 ft. long. I have heard, however, in conversation, very much greater lengths assigned to this tank, not 50 ft., but 50 metres; in this case the question of the relative quantities involved, and the relative sizes of working becomes one of an entirely different scale to what is the case if the whole can be done in a tank of only 40 ft. or 50 ft. long with wells at either end of only 40 ft. or 50 ft. deep. The real difficulty which occurs to my mind is that which Mr. Froude has brought out. It is necessary during the time the resistance is to be measured that the motion should be steady in its character, and that the ship should be generating at a uniform rate the waves to which the greater proportion of the resistance is due. Now, that steady period is, in Herr Wellenkamp's case, preceded by a brief period of very violent motion in which a considerable velocity is given to the model in a comparatively short space of time, and it will want very careful investigation of the results of experiments of this kind before we can feel at all sure that the effects of that violent motion have entirely subsided before the so-called steady state has come; in fact, what Mr. Froude has said in his last words shows us that it is not possible to suppose that all the effects of the violent starting of the motion have subsided when the model has traversed only a length and a half in the steady condition. Those

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points, it seems to me, would need very careful examination and very careful investigation before we could feel the same confidence in the results deduced from a tank of this size, and from working in this manner, that all, I think, are now assured they can place in results obtained by the method that was inaugurated by Mr. Froude's father so many years ago.

Herr H. WELLENKAMP (Member): My Lord and Gentlemen, concerning the necessary size of tanks about which Dr. Glazebrook wished to know, I recommended in the paper a length of four and a half times the length of the largest models to be tested. Thus, for models of an average length of 14 ft. representing the biggest ships to a ¼ in. scale, as mentioned by Mr. Froude in the discussion, the tank would not exceed 65 ft. in length. When starting on these experiments, I used very small models, because I wished to work as cheaply as possible. But after having proved that a short run is amply sufficient, I proceeded to use larger models, of one-twentieth full size. These models, when representing large ships, measure 30 ft. and

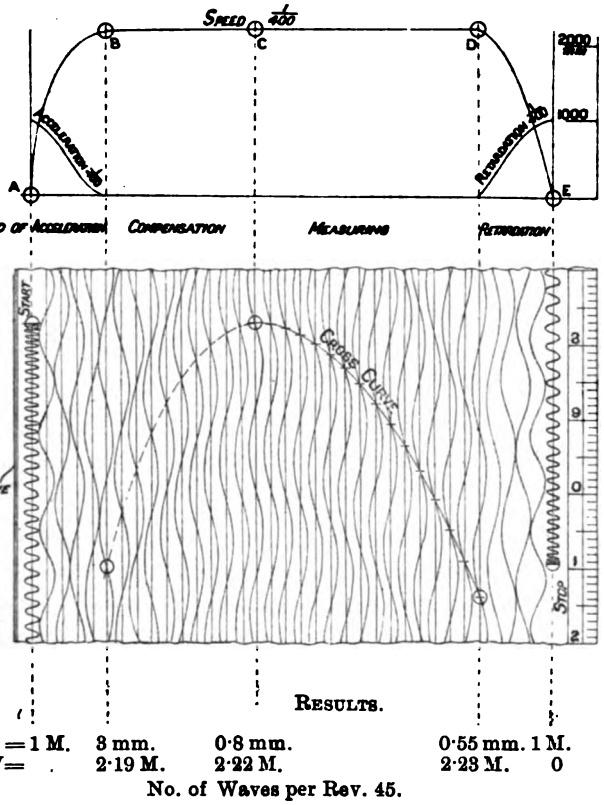

Fig. 5.
SPEED AND CHANGE SPEED.
$\frac{1}{100}$ scale.

Fig. 6.
ORIGINAL FORM OF DIAGRAM ON DRUM SURFACE.

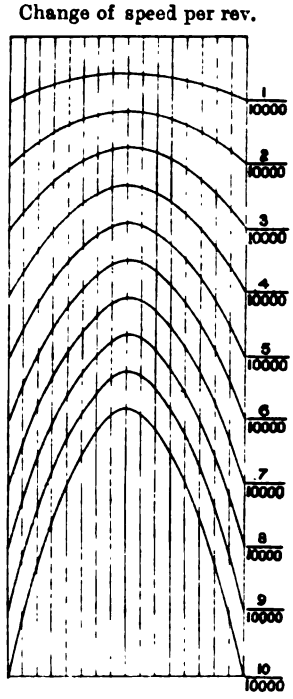

Fig. 7.
CURVATURE OF CROSS CURVES on the Drum Surface due to Change of Speed (natural size).

more, and have to be built cheaply and rapidly. Their weight is upwards of 4 tons—that is, nearly fifteen times that of the models usually met with in experimental work. It will be obvious that there are numerous advantages to be derived from using this size of models, as much for accuracy of results of towing and screw experiments, as for proper conclusions to be drawn therefrom. There is no reason why one should return to small models if it were not, perhaps, on account of the expense of building the tank. The tanks of the Hamburg Government and of the Imperial German Navy will be built for the large size of models, the length of these tanks being about 150 ft.

Mr. Froude called in question the accuracy of my method, pointing out that the model could not attain uniform speed in the short period of acceleration which I allowed. I agree with that opinion within certain limits, and I took this point into account, as may be seen from the diagram (Fig. 5), showing the velocities during the whole run by the curve A B C D E. The velocity increases very

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quickly under the influence of the starting weight up to point B. Then the model moves at a nearly uniform speed up to point C—that is, a distance at least equal to the length of the model itself, and, in my opinion, sufficient for it to finally settle down to a really uniform speed. This transitory state may be called the "period of compensation." Between C and D the measuring is carried out; beyond D the brake begins to act. The same process is shown again in Fig. 6, which represents the original form of diagram as a portion of the drum surface with the spiral wavy-line. It shows the distances travelled by the model for every hundredth part of a second—*i.e.*, the velocities shown here are a hundred times larger than in the first diagram. Near the starting-point A, where the velocity is very small, the waves are correspondingly very short. After each revolution of the drum—*i.e.*, after each full metre of run, the velocities are increased up to the end of the period of acceleration at the point B. From here to C and D the lengths of the single waves seem to be constant, but there is an infinitesimal increase or decrease which appears at once by drawing cross-curves through the crossing points of adjacent waves. The whole of the increase or decrease of all the waves of one revolution is shown by the exceedingly small differences of successive turns of the spiral line—in other words, by the more or less remarkable curvature of the cross-curves, which is mentioned in the paper as an indication of acceleration or retardation. This curvature is really a wonderful test of the variation of speed, given to full size and by a method admirably adapted for an exceedingly delicate measurement, as shown in Fig. 7. There is no better or simpler means of showing automatically the slightest variations of speed than by the cross-curves through the wavy line; and, at the same time, there is no other instance where the highest degree of accuracy in measurement of speed and variation of speed is of more importance—and, we may add, where it is more urgently needed—than in testing model resistances. Now, we have here for the first time a means of measuring change of speed with an accuracy of a ten-thousandth part of a metre, and of a second, at any part of the run, which is more than Mr. Froude imagined to be possible. As I do not wish to take up any more of your time, I should prefer to give further explanations in writing, if you will allow me.

The PRESIDENT: Certainly. I think we are all very much indebted to Herr Wellenkamp for coming here and reading his very interesting paper, and I think we should all join in giving him our best thanks for having presented it to the Institution, and for the interesting discussion that it has evoked.

Herr WELLENKAMP sent the following additional written reply:—With regard to the accuracy of speed measurement mentioned above, it will be evident that my method is not only adapted to test the speed and the corresponding resistance directly, but also, if the resistance does not exactly equal the acting towing force, it enables one to calculate the slightest amount of unbalanced force from the small change of speed produced by this force. For instance, in Figs. 5 and 6, on the previous page, the speed at the end of the acceleration (at point B) is 2·19 M., at the end of the period of compensation at point C the speed has increased to 2·22 M. Whether the speed during the next period of measuring was uniform or varying, and which unbalanced force caused the variation in speed, can be tested by the analysis of the result of the trial from the diagram in Fig. 6, as shown in the Table on the opposite page.

This example deals with an average acceleration of less than a three-thousandth part of the circumference of the drum, a quantity which was mentioned by Mr. Froude in the discussion. I hope this example will best explain that the infinitesimal quantities of change speed, even during a very short period, when shown by this refined method on the drum surface, are not at all too small for satisfactory measurement. It seems necessary to state that errors of observation are entirely avoided, and that the slightest disturbances due to adventitious circumstances are detected at a glance. Thus, the results of even a single trial, if carried out satisfactorily, are at any rate trustworthy. As to the violent motion in the brief period of acceleration, which Dr. Glazebrook believes to be the cause of disturbances in the measuring period, I can only refer to the diagrams, which show nothing of what may be called violent motion, but a smoothness of motion like that of a body falling freely and with diminished velocity. Of course, there are ready means of preventing pitching during the period of acceleration, because the oscillations would not cease during the whole run if they once occurred. The arrangement devised in Mr. Parsons'

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tank to avoid yawing is certainly to be recommended. I also use a leading wire in some cases, but a long bowsprit has, nevertheless, great value in keeping down the friction of the wire. Now, there is one very important question to deal with, which I only mentioned in a few words in my reply to the discussion.

TABLE.

Weight of model, towing weight, wire, drum ... = 2020 kg.
Mass (m) ... ... $\frac{2020}{9{\cdot}81}$ = 205·9

Towing force (p) ... ... = 16 kg.
Number of waves per revolution (n) ... = 45

Readings from Drum Surface. Mm.	Differences. V_n = speed per n waves. Mm.	Differences. γ_n = change of speed per n waves. Mm.
77·2	—	—
77·3	1000·1	—
77·9	1000·6	0·5
78·9	1001·0	0·4
80·2	1001·3	0·3
81·8	1001·6	0·3
83·5	1001·7	0·1
85·4	1001·9	0·2
87·7	1002·3	0·4
90·5	1002·8	0·5
93·6	1003·1	0·3
97·1	1003·5	0·4
00·9	1003·8	0·3
05·0	1004·1	0·3
09·3	1004·3	0·2
13·9	1004·6	0·3

Mean values : V_n 1,003·4 mm. γ_n 0·3 mm.
+ 0·45 + 0·45
V per sec. = 2·223 M. γ per sec. = 0·66 mm.
Unbalanced force $= m.\gamma$ = 136 gram.
Resistance of model $= p - m.\gamma$ = 15·864 kg.

I mean the objection raised by Mr. Froude, that the model must have travelled two lengths with the towing weight only, before uniform speed can be expected. Mr. Froude's opinion is strengthened by the theoretical principle that there may be during this travelling a continuous change in position and height

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of the echoes of the bow transverse wave, due to the amount of energy passing from bow to stern. Nevertheless, I must state that, so far as I could measure, there was no perceptible change which could have been attributed to the influence of alteration of transverse-waves after having passed the first length —what I called the period of compensation. It will be the object of further careful investigation to give a complete explanation of these facts; I merely point out the way of solving this problem. At any rate, if one should insist upon having the model cover a distance of two lengths instead of one with the towing weight alone—whether or not it may be necessary from a practical point of view—I do not think this is of great importance as affecting the principle of the method. In this case the tank would be not four and a half times the length of the model, but five and a half times, and would still be, as before, extremely short.

In conclusion, it may be mentioned that I was without any knowledge of the apparatus which Beaufoy or William Froude or Mr. Parsons had adopted. What Dr. Glazebrook said is quite to the point—viz., that the whole apparatus is nothing but an Atwood machine adapted to the purpose in hand by the following modifications: (1) The starting weight is built up as a chain, the weight of which dwindles away during the period of acceleration to zero. (2) The starting weight works by a separate thread, and is automatically detachable. (3) The chief feature consists of a special registering apparatus, which is conveniently combined with the only working mechanism—*i.e.*, the pulley—which has here the form of a drum. I maintain that these three points are indispensable in obtaining a satisfactory result. As far as I know, Beaufoy's apparatus was not of that kind, and I fully understand William Froude's criticism of his own results with this system, as Mr. R. E. Froude has told us. I must beg that this criticism of Beaufoy's method shall not be applied to mine, having regard to the numerous distinctive features of the two. I have to thank the members present at my lecture for the kind way in which they have received this paper, and I hope that this new method, of a less elaborate and costly nature, will serve as a powerful aid to progress in ship design.

Written Contribution to the Discussion.

Mr. R. J. Walker (Member): As I was unable to be present at the discussion on Herr H. Wellenkamp's paper, I beg to be allowed to submit one or two remarks thereon. Herr Wellenkamp states, that it was Sir William H. White's paper, read before the Naval Architects in 1904, which first induced him to deal with experimental tank problems. Sir W. H. White, in his paper, referred to the arrangements for testing resistances of models of turbine-propelled vessels devised by Mr. Parsons. The arrangement of tank now at the Turbinia Works, introduced by Mr. Parsons some eight years ago, is substantially the same as that described by Herr Wellenkamp, so far as obtaining the resistance of the model is concerned, with certain slight differences in the mechanical arrangement and recording apparatus—that is to say, the resistance of the model is ascertained by means of a falling weight, with an additional weight for accelerating the speed of the model at starting. The length of the tank to which I refer (between points at which records are taken) is 101·3 ft., or 1–60th part of a knot, with a distance of 50 ft. from point of release of model, for acceleration, to the first point of contact, the depth of the tank being 12 ft.

As Herr Wellenkamp rightly states, the chief consideration in dealing with towing experiments is that the model should follow a direct course straight forward and be totally free from deviation and disturbances, and with, as far as possible, a uniform velocity. The problem of keeping the model perfectly straight throughout the length of the run when towing from the bow by means of a very fine wire is a very difficult one, and although the fitting of bowsprits considerably reduces the tendency for the model to "yaw," yet these bowsprits do not altogether overcome this difficulty. In the later experiments, vertical guides have been fitted to the models, one at each end, with a trunnion fitted between each. At each end of this trunnion small wheels are fitted which run on a fixed wire connected from one end of the

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tank to the opposite end, the trunnions being free to move on these guides in a vertical direction to allow for sag of wire. By this means a straight course is ensured. The wire used for towing is a very fine one of phosphor bronze, 35 L.S.G., or ·0084 in. The whole of the pulleys in connection with the gear are mounted in ball bearings, so that the friction is reduced to a minimum. All the gear can be tested from time to time for friction, quite independently of the model, and a correction in the records made accordingly. The whole cost of the tank and apparatus is comparatively very low. The results obtained appear to be very reliable, and compare very closely with results obtained from other large experimental tanks in this country and from actual trial trip results of vessels themselves.

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TWO NOTES ON SHIP CALCULATIONS.

By W. S. ABELL, Esq., R.C.N.C.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 10, 1908; Professor J. H. BILES, LL.D., Vice-President, in the Chair.]

THE first note consists of a discussion of the various methods used in ship calculations, including a new rule for finding areas, &c., with a considerably less number of ordinates than is usually employed.

The second note gives a method of constructing an approximate metacentric diagram, within an error of about 1 per cent., when only the length, breadth, draught, displacement, and tons per inch are known.

METHODS OF SHIP CALCULATION.

As a first step in finding the approximate area of a figure bounded by a curve on the one side and by straight lines at the other side and ends, a function can always be found which will coincide with the given curve in any desired number of points.

In general the form of the function is given by—

$$y = a_0 + a_1 x + a_2 x^2 + \ldots + a_{2n-1} x^{2n-1}.$$

This form involves $2n$ constants, which can be so determined that the function and the given curve coincide in $2n$ points. In the process of finding the area of the given figure, the assumption usually made is that the area is the same as that under the function which coincides with the given curve in $2n$ points.

It is fairly obvious that, unless the given curve is, mathematically speaking, "smooth," *i.e.*, free from sudden changes of slope and curvature, the difference between the true and approximate areas will be large; so that, in such cases, the area cannot be approximately calculated by a function *passing through* the point where this sudden change of slope occurs, *i.e.*, this point should be the termination of the approximate integration.

Ship curves are of two general kinds—the horizontal sections or water planes, which are "smooth" curves, and the vertical transverse sections, which are for

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the lower portion of the depth "not smooth," having a very sudden change of slope at the turn of the bilge. It therefore follows that, to avoid error, the approximate integration can only be carried out from the top waterline down to the turn of the bilge. This limitation is in accordance with usual practice, the lower portion, or keel appendage, as it is termed, being separately calculated, and for like reasons the forward and after extremities of the ship are treated in the same manner.

In order to calculate the main portion of the displacement, bounded vertically by the top waterline and a waterline near the turn of the bilge and horizontally by sections, near, but not at, the ends of the ship, the usual practice is to divide the length and depth into a comparatively large number of equal parts, the number for the length being about 20 and for the depth about 6. The assumption usually made is that, for each pair of spaces which include three ordinates, the function approximating to the given curve is a second degree parabola,

$$y = a_0 + a_1 x + a_2 x^2.$$

It is contended that, as the function is only assumed to coincide with the given curve over a small portion of the figure, the error introduced is smaller than would be the case if the function were assumed continuous over a greater length. This contention does not appear altogether logical, because great care is taken to prevent irregularities in the form of a ship, *i.e.*, to make the curves "smooth" or fair, and it seems reasonable to infer that the lines of a ship are given fairly accurately by a general function of the type indicated.

The first practical recognition of this fact appears to have been due to Tchebyscheff, who gave a general formula for the lines of a ship, and who also worked out his rule for approximate integration on the same assumption. This rule, as is well known, fixes the ordinates to be used for integration in such a manner that the area of the curve is given by the sum of the ordinates.

To consider the general case: In the function taken there are $2n$ conditions to be satisfied, and this can be done in a number of ways, *e.g.*:—

(*a*) The positions of the ordinates may be fixed, in which case the number of ordinates will be $2n$, and the quantities which are determined from solution of the equations involved are the multipliers of the ordinates, which are $2n$ in number. A special case of this type is the ordinary Simpson's rule, in which the ordinates are fixed, one at each extremity of the area, and the remainder at equal intervals.

(*b*) The multipliers may be assumed equal to each other, which is one condition; there are therefore $(2n-1)$ more conditions to fulfil, which can be satisfied by determining the positions of $(2n-1)$ ordinates. This is Tchebyscheff's rule; the multipliers all being equal, the area is, therefore, given by the sum of the ordinates.

(*c*) As $2n$ conditions have to be fulfilled, and as a reduction in number of ordinates generally means a reduction in calculation, the conditions can also be satisfied by

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taking n ordinates and n multipliers, the positions of the ordinates and the values of the multipliers being found from the general equations. It follows that by this method n ordinates should give the same approximate area as $(2n-1)$ Tchebyscheff ordinates, *i.e.*, practically half the number.

As regards this third method, Professor Burnside, F.R.S., of the Royal Naval College, Greenwich, first calculated for me the positions of the ordinates and the values of the multipliers, and afterwards found that the results had been previously worked out by Gauss and published in Todhunter's Functions of Laplace, Lamé, and Bessel in 1875. The table at the end gives the positions of the ordinates and the values of the multipliers for different numbers of ordinates, as originally calculated by Gauss; Professor Burnside checked the results up to five ordinates. The theoretical considerations for finding the multipliers and positions of ordinates are given in Appendix I.

Judging from previous experience with Tchebyscheff sections, it was considered that nine of these ordinates would be sufficient for all, or nearly all, calculations, and on this basis five Gauss ordinates should give the same degree of accuracy.

The multipliers are rather cumbersome, but it was found that these could be replaced by an approximate form, *viz.*: 5, 10, 12, 10, 5, without any appreciable loss of accuracy. Further, with this rule, sections occur at mid length, at approximately quarter length, and near the ends, giving a good general idea of the form of the ship.

An examination of the accuracy of Simpson's, Tchebyscheff's, and Gauss's methods was then undertaken. The form of ship used was one which had been worked out a large number of times by several individuals, so that it was possible to practically eliminate the personal error in the calculations. Further, the results were compared in particular cases with integrator calculations, which give absolutely accurate results.

In ship calculation, the process is arranged in such a manner that, to obtain the desired result, the integration is reduced to finding the area of a curve of some kind. The starting point of all calculation is from the ordinates of a waterline or transverse section, and the fact that the final curve is seldom, if ever, plotted, may lead to serious error, because it is not known if the shape of the curve involves any serious change of slope.

To illustrate the point that all calculations reduce in effect to finding the area of a curve, suppose it is desired to calculate the vertical position of the centre of buoyancy. The arrangement of the ordinary displacement sheet is such that the sum of vertical columns gives functions representing the area of water planes. To obtain the vertical position of the centre of buoyancy, each function of water-plane area is multiplied by its distance below the top water plane, the products being integrated by Simpson's multipliers. The sum of these functions then gives the total moment of the displacement below the top waterline. The essential part of the calculation is

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that a curve of moment of areas of water planes has been constructed, and afterwards integrated by Simpson's rules.

The first step was, therefore, to ascertain if any of the derived curves used in the process of approximate integration would have any serious irregularity, bearing in mind the fact that the original curve was supposed "smooth."

The underlying integrations for all the usual calculations are given in Appendix II., and the shapes of the various derived curves are shown in Figs. 1 and 2 (Plate XXVII.).

From the theoretical considerations given, and from the figures, it will be seen that in only one case was there any probability of departure from the "smooth" condition, and that was, unfortunately, in a very important one, viz., in calculating the height of the transverse metacentre above the centre of buoyancy.

It therefore follows that for all calculations, except that referred to, a function of the general form indicated, should give the results as closely as by the usual methods.

This probability was checked by actual calculation, taking the usual degree of accuracy, *i.e.*, that the true and approximate results should agree within a maximum error of one-half per cent. For all calculations, except the one specified, the desired result was given by the following arrangements of ordinates :—

Simpson's rule	...	13	Transverse	sections	...	5	Water	planes.
Tchebyscheff's rule	...	9	„	„	...	5	Simpson water	planes.
Gauss' rule	... ...	5	„	„	...	5	„	„
„ „	... ...	5	„	„	...	3	Gauss	„

The fact that five Gauss transverse sections gave as good a result as nine Tchebyscheff sections shows that the theory advanced is confirmed by fact.

To apply Gauss' rule to practice, it is to be observed that the Gauss water planes, as also Tchebyscheff's, do not occur at desirable places, *e.g.*, no ordinate occurs at the top water plane, and it is desired to know nearly all the properties of this particular plane. It therefore appears advisable, for this and other reasons, to have the water planes evenly spaced, and consequently the method to adopt appears to be Gauss transverse sections in association with Simpson water planes.

It has been stated before that one Gauss section is at the middle of length, but the four other sections will have to be constructed. It is suggested that, in addition to the usual sections shown in a body plan, these Gauss sections should be drawn in dotted, as they afford a ready means of calculation of displacement, &c., when the ship is in a damaged or otherwise unusual condition.

Before devising a tabular form for the displacement sheet on these lines, the question of the calculation of the transverse metacentre was considered. It has been shown in Appendix II. that if n ordinates are necessary to find the approximate area of a water plane, then, for the same degree of accuracy, $(3\,n - 1)$ ordinates would

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theoretically be required to find the transverse inertia of the water plane. The statement above that five Gauss sections are necessary for length, refers, of course, to calculation of displacement, centre of buoyancy, &c.; but, if the water plane only is being considered, three Gauss ordinates would probably give good results. It therefore appears that, apart from considerations of great slope, at least eight Gauss ordinates are required to give the transverse inertia.

As a result of calculation, it was found that, in order to get the calculation sufficiently accurate, it was necessary to use either—

Simpson's rule...	11 or 13 ordinates.	
Tchebyscheff's rule	10	„
Gauss' rule	8	„

It is much easier to use Simpson's rule, with 11 ordinates evenly spaced with or without 2 half ordinates at the ends, than to construct new Gauss sections. The reason of the apparent inconsistency appears to be due partly to the fact that a Simpson ordinate occurs near each discontinuous point, and partly because the area of the part of the curve before and abaft the discontinuous points is a small fraction of the total area.

The accompanying Table shows a displacement sheet embodying Gauss' rule, which shows a considerable saving in labour without any loss in accuracy.

As regards stability at large angles, there is no *a priori* reason why Gauss' rule should not apply. Hitherto, the minimum number of sections used has been 10 Tchebyscheff, or 11 to 13 Simpson; 5 Gauss sections should, therefore, in accordance with the previous remarks, suffice.

In order to further check the accuracy of Gauss' rule, a quite distinct ship form from the previous one was taken, and the stability carefully worked out in the usual manner. A new body was made with Gauss sections and cross curves of stability found by means of the integrator. The actual curve of stability so obtained agreed, within the usual limits of error, with the curve found by using the greater number of sections, which latter curve was also checked in certain spots by direct calculation. This test of Gauss' rule, which is certainly more searching than any previous one, may be considered as sufficiently good to justify its general adoption.

The labour of stability calculation is obviously considerably shortened—the actual time involved in working out 4 cross curves, or 16 actual determinations of displacement and arm of the righting couple, being only $2\frac{1}{2}$ hours. There was, of course, in addition, the time required to trace the body and to plot the cross curves; but, in general, it may be said that a complete determination of the statical stability curve is possible with about 3 hours' work.

There is one further point about the displacement sheet, and that is regarding the calculation of the keel appendage, which is shown on the left of the sheet. In order to render Gauss' rule more accurate, it is desirable to imagine the deadwood and cut-up

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T

... ... Ft. In. 4 6

ACCORDING TO GAUSS' RULE.

Number of Ordinate	Vertical Sections. Function of Area.	Multiples of Area.	Multipliers for Levers.	Difference of Moments.	Centre of Flotation and Longitudinal Metacentre. Function of Ordinates of L.W.L.	Sum or Differences of Functions of Ordinates.	Lever.	Moment.	Lever².	Moments of Inertia.
1	—	—	—	—	24·0	75·0	—	—	·82	61·5
	19·3	96·5	·91	5·94						
2	—	—	—	—	200·0	424·0	—	—	·29	123·0
	96·6	966·0	·54	11·83						
3	—	—	—	—	340·8					
	82·2	1946·4								
4	—	—	—	—	224·0	24·0	·54	13·0	—	—
	97·7	977·0	·54							
5	—	—	—	—	51·0	27·0	·91	24·6	—	—
	21·9	109·5	·91							
		4095·4		17·77	839·8			37·6		184·5

Dis 4095·4

45·6

C. I

C. I

= 8·07 ft.

Area L.W. Plane = $2 \times 839{\cdot}8 \times \frac{320}{42}$ = 12,800 sq. ft.

Area after appdge. = 95 sq. ft.

Total area = 12,895 sq. ft.

C. F. aft Mid. Sec. = 7·02 ft.

C.G. aft appdge. aft Mid. Sec. = 165 ft.

Total C. G. aft = 8·2 ft.

Longl. M. I. about Mid. Sec. = $12{,}800 \times \frac{184{\cdot}5}{839{\cdot}8} \times (160)^2$

= 71,950,000

I about Mid. Sec. of aft appdge. = 2,530,000

Deduct A h^2 for I about C. F. = — 880,000

I about C. F. = 73,600,000

Longl. B. M. = I ÷ V = 368 ft.

	B. ft Sec.	Moment abaft.
1	·7	3,745
3	·55	190
5	4·5	1,233
7	—	—
9	7·0	487
11	—	—
13	7·6	2,129
15	0·1	— 292
17		
19		7,492
21		683
	1·2	6,809

TOTAL DISPLACEMENT = 5,708 tons.
C. B. = 8·7 ft. below L.W.L.
= 1·2 ft. aft Mid. Sec.

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forward filled in to a thickness equal to the width of keel, down to the straight line of keel produced; this being done both for the main piece of the displacement and for the keel appendage especially. With this device Gauss' rule can be used to calculate the keel appendage, and the triangular-shaped parts can be easily deducted in the final summary table.

Approximate Construction for the Metacentric Diagram.

The form of construction of the metacentric diagram which is usually employed for warships, the details of which are generally understood, is shown in Fig. 3 (Plate XXVII.).

The purpose of this approximation is to enable a fairly accurate (generally within about one per cent.) estimate of the position of the transverse metacentre relative to the ship to be obtained when only the principal dimensions of the ship are known.

Let L be the length, B the beam, and D the draught in feet, Δ the displacement in tons, and T the tons per inch, then—

$$\frac{\Delta \times 35}{L \times B \times D} = b, \text{ the "block co-efficient."}$$

$$\frac{T \times 420}{L \times B} = a, \text{ the "waterline co-efficient."}$$

In the initial stages of a design, L, B, D and Δ are generally roughly fixed, and it is desirable to know if the beam is sufficient to give the required metacentric height. The usual form of approximation is to give the value of B M, *i.e.*, the distance of the metacentre above the centre of buoyancy, as—

$$B\,M = \text{constant} \times \frac{B^2}{D}.$$

It appears reasonable to suppose that if the value of a is known, a closer approximation would be possible.

In the case of an actual ship, information is generally given regarding the variation of displacement with draught from which a can be determined, and in the design stage a can be inferred from a previous ship.

An investigation, which included a large number of types of warships, was made as to the relation between the transverse radius of gyration of the water line area and the mean breadth, which is $a \cdot B$, and it was found that, covering a very large range of type,

$$\frac{K}{B} = \cdot 2\,a + \cdot 1, \qquad (1)$$

where K is the radius of gyration. The material for merchant ships was not

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available, so the constants in the approximate relation could not be determined; but it would doubtless be the form—

$$\frac{K}{B} = m \cdot a + n.$$

The value of B M is given by—

$$B\,M = \frac{A\,K^2}{V} = \frac{a}{b}\cdot\frac{L}{L}\cdot\frac{B}{B}\cdot\frac{K^2}{D} = \frac{a}{b}\,\frac{K^2}{D},$$

and can be at once determined from the value of K obtained from the formula.

The depth h of the centre of buoyancy below the waterline is given by Morrish's rule, as follows :—

$$h = \tfrac{1}{3}\left(\frac{\Delta}{12\,T} + \frac{D}{2}\right)$$

$$= \tfrac{1}{3}\left(\frac{b \times L \times B \times D \times 420}{35 \times 12 \times a \times L \times B} + \frac{D}{2}\right)$$

or

$$\frac{h}{D} = \frac{b}{3a} + \frac{1}{6}. \qquad (2)$$

Hence, knowing the values of B M and h, the position of the metacentre relative to the ship is obtained for the particular draught D. If it is desired to know the position of the metacentre at any other draught, the new position of the centre of buoyancy and the new value of B M must be found.

The locus of the centre of buoyancy is generally a straight line, and, in order to determine the locus, it is only necessary to know one point, and the slope of the line, which is given by—

$$\tan\theta = \frac{A\,h}{V},$$

where θ is the inclination to the horizontal, A is the area of the water plane and V the volume of displacement. Substituting for A, V, and h—

$$\tan\theta = \frac{a\,L\,B\,D}{b\,L\,B\,D}\left[\frac{b}{3a} + \frac{1}{6}\right]$$

$$= \frac{1}{3} + \frac{1}{6}\cdot\frac{a}{b}. \qquad (3)$$

From which equation it follows that, since the curve of buoyancy is a straight line for considerable variations of draught, the ratio of a to b is constant for the same region. The position of the centre of buoyancy is thus determined for the new draught.

As regards the new value of B M—

$$B\,M = \frac{I}{V} = \frac{I}{\Delta \times 35},$$

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where I is the inertia of the water plane, a quantity which varies little with alteration of draught. The displacement, on the other hand, is a direct linear function of the draught, and can be written $\Delta = p\,D + q$. If values are substituted in the above equation it becomes—

$$B\,M\,(p\,D + q) = \text{Constant},$$

or—

$$B\,M \times D = \text{Constant}. \qquad (4)$$

Hence, knowing the value of the B M for the original draught D, it can be determined for any other draught by equation (4).

The approximate metacentric diagram is thus fully determined by means of the four equations. Fig. 3 (Plate XXVII.) shows for a particular case the approximate diagram, dotted as well as that directly calculated, which is shown in full lines.

While on the subject of the metacentric diagram, a curious point presented itself, which does not appear to be generally known, and that is in respect of the lowest point of the metacentre. As the draught increases, the centre of buoyancy rises and the value of B M diminishes. Let $\delta\,\Delta$ be the alteration in displacement and $\delta\,B\,M$ the alteration in B M, then, the rise of the centre of buoyancy

$$= \frac{h\,\delta\,\Delta}{\Delta},$$

and, from the assumptions used in connection with formula (4),

$$B\,M \times \Delta = \text{Constant},$$

whence, differentiating,

$$B\,M\,\delta\,\Delta + \Delta\,\delta\,BM = 0,$$

or—

$$\delta\,B\,M = -B\,M\,\frac{\delta\,\Delta}{\Delta}$$

therefore the rise of the metacentre is equal to

$$\frac{h\,\delta\,\Delta}{\Delta} - B\,M\,\frac{\delta\,\Delta}{\Delta} = (h - B\,M)\,\frac{\delta\,\Delta}{\Delta}. \qquad (5)$$

From equation (5) it follows that, if h is greater than B M, the metacentre rises, or that the metacentre is at its lowest point when $h = B\,M$, *i.e.*, when the metacentre is in the waterline. This condition would be expressed by saying that, in the diagram constructed as in the example given, the metacentric locus should be horizontal at the point where it cuts the 45° line. A large number of metacentric diagrams have been inspected, and it is found that this property is generally true, the lowest point, however, occurring generally just before the locus crosses the line.

In conclusion, I have to express my thanks to Professor Burnside, of the Royal

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Naval College, Greenwich, for considerable assistance in connection with the first part of this paper.

RULES FOR APPROXIMATE INTEGRATION.

Rule.	Simpson.		Tchebyscheff.		Gauss.		
Degree of Function.	Number of Ordinates.	Multipliers.	Number of Ordinates.	Positions of Ordinates.*	Number of Ordinates.	Positions of Ordinates.*	Multipliers.
1st ...	2	$\frac{1}{2}$ (1 . 1 .)	—	—	1	0	1
2nd ...	3	$\frac{2}{6}$ (1 . 4 . 1)	2	± ·5774	—	—	—
3rd ...	4	$\frac{3}{8}$ (1 . 3 . 3 . 1)	3	0 ± ·7071	2	± ·5774	$\frac{1}{2}$
4th ...	5	$\frac{4}{90}$ (7 . 32 . 12 . 32 . 7)	4	± ·1876 ± ·7947	—	—	—
5th ...	6	$\frac{5}{288}$ (19 . 75 . 50 . 50 . 75 . 19)	5	0 ± ·3745 ± ·8325	3	0 ± ·7746	$\frac{8}{18}$ $\frac{5}{18}$
6th ...	7	$\frac{6}{840}$ (41 . 216 . 27 . 272 . 27 . 216 . 41) or Weddle's approximation $\frac{6}{20}$ (1 . 5 . 1 . 6 . 1 . 5 . 1)	6	± ·2666 ± ·4225 ± ·8662	—	—	—
7th ...	8	$\frac{7}{17280}$ (751, 3577, 1323, 2989, 2989, 1323, 3577, 751)	7	0 ± ·324 ± ·530 ± ·884	4	± ·3400 ± ·8611	·3261 ·1739
8th ...	9	$\frac{8}{28350}$ (989, 5888, −928, 10496, −4540, 10496, − 928, 5888, 989)	—	—	—	—	—
9th ...	10	$\frac{9}{89600}$ (2857, 15741, 1080, 19344, 5778, 5778, 19344, 1080, 15741, 2857)	9	0 ± ·1679 ± ·5288 ± ·6010 ± ·9116	5	0 ± ·5385 ± ·9062	·2844 ·2393 or $\frac{12}{45}$ ·1185 $\frac{10}{45}$ $\frac{5}{42}$ approx.
10th ...	11	$\frac{10}{598752}$ (16067, 106300, −48525, 272400, −260550, 427368, − 260550, 272400, −48525, 106300, 16067)	—	—	—	—	—
13th ...	—	—	—	—	7	0 ± ·4058 ± ·7415 ± ·9491	·2090 ·1909 ·1399 ·0647

* Positions of ordinates are given as a fraction of half the length, measured from middle of length.

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APPENDIX I.

Theory of Approximate Integration.

As explained in the paper, in order to find the approximate area under a curve, it is assumed that the equation of the curve is given by a function of the type—

$$y = a_0 + a_1 x + a_2 x^2 + \ . \ . \ . \ + a_{2n-1} x^{2n-1} \qquad (1)$$

The problem is to determine the area under this function in terms of a certain number of ordinates of the function.

The equation of the curve, as written above, contains $2n$ constants, which can be determined in such a manner that the function will coincide with the given curve in $2n$ points. The assumption made for purposes of approximate integration is that the area under the chosen function is equal to the area under the given curve, which approximation can only be good as long as the given curve is fairly "smooth."

The area can be expressed in terms of certain ordinates and certain multipliers of such ordinates. Let p_1 be the multiplier and y_1 be the ordinate corresponding to a value $x = x_1$; then, if A be the area,

$$A = p_1 y_1 + p_2 y_2 + \&c.$$

The quantities that have to be determined are the values of the multipliers p_1, and the positions of the ordinates, *i.e.*, the values of x_1.

The function (1) contains $2n$ constants, and therefore the total number of quantities which can be found are $2n$, or, in other words, *the sum of the total number of ordinates and the total number of multipliers must be equal to* $2n$.

The method of determination of p_1, p_2, &c., and x_1, x_2, &c., is as follows :—

$$A = \int y \, dx,$$

and, without loss of any generality, the limits of the integration can be put as $+1$ and -1.

$$\therefore A = \int_{-1}^{+1} y \, dx = \int_{-1}^{+1} (a_0 + a_1 x + a_2 x^2 + \ . \ . \ . \ + a_{2n-1} x^{2n-1}) \, dx$$

$$= \left(a_0 x + a_1 \frac{x^2}{2} + a_2 \frac{x^3}{3} \ . \ . \ . \ + a_{2n-1} \frac{x^{2n}}{2n} \right)_{-1}^{+1}$$

$$= 2 \left[a_0 + \frac{a_2}{3} + \frac{a_4}{5} \ . \ . \ . \ . \ + \frac{a_{2n-2}}{2n-1} \right] \qquad (2)$$

But, by assumption,

$$A = p_1 y_1 + p_2 y_2 + p_3 y_3 + \&c.$$

and, substituting the value of y_1 in terms of x_1,

$$\begin{aligned} A = {} & a_0 [p_1 + p_2 + p_3 + \&c.] \\ & + a_1 [p_1 x_1 + p_2 x_2 + p_3 x_3 + \ . \ . \ . \ . \ ] \\ & + a_2 [p_1 x_1^2 + p_2 x_2^2 + \ . \ . \ . \ . \ ] \\ & + a_3 [p_1 x_1^3 + \ . \ . \ . \ . \ ] \\ & + \ . \ . \ . \ . \end{aligned} \qquad (3)$$

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Now the value of the area found in the two ways is the same, and therefore the right-hand sides of equations (2) and (3) must be the same whatever the values of a_0, a_1, a_2, &c., whence

$$\left.\begin{aligned} p_1 + p_2 + p_3 + \ldots\ldots &= 2 \\ p_1 x_1 + p_2 x_2 + p_3 x_3 + \ldots\ldots &= 0 \\ p_1 x_1^2 + p_2 x_2^2 + p_3 x_3^2 + \ldots\ldots &= \tfrac{2}{3} \\ p_1 x_1^3 + \ldots\ldots &= 0 \\ p_1 x_1^4 + \ldots\ldots &= \tfrac{2}{5} \end{aligned}\right\} \qquad (4)$$

and so on.

This gives a series of equations whence the values of p_1, p_2, &c., and x_1, x_2, &c., can be determined. There are $2n$ equations, and from these only $2n$ quantities can be determined.

To take the case of Simpson's rule, the ordinates are equally spaced, and, therefore, if there be an odd number of ordinates, one will occur at $x = 0$, and the others will be in pairs symmetrically placed about the middle of length, *i.e.*, with $(2n-1)$ ordinates—

$$\begin{aligned} x_1 &= -x_{2n-1} = 1; \\ x_2 &= -x_{2n-2} = \frac{n-2}{n-1}; \\ x_3 &= -x_{2n-3} = \frac{n-3}{n-1}; \\ &\ldots\ldots\ldots; \\ x_n &= 0. \end{aligned}$$

The second, fourth, sixth, &c., of equations (4) then give—

$$\begin{aligned} &(p_1 - p_{2n-1})\,x_1 + (p_2 - p_{2n-2})\,x_2 + \ldots\ldots + (p_{n-1} - p_{n+1})\,x_{n-1} = 0 \\ &(p_1 - p_{2n-1})\,x_1^3 + (p_2 - p_{2n-2})\,x_2^3 + \ldots\ldots + (p_{n-1} - p_{n+1})\,x_{n-1}^3 = 0 \\ &\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ &(p_1 - p_{2n-1})\,x_1^{2n-3} + (p_2 - p_{2n-2})\,x_2^{2n-3} + \ldots + (p_{n-1} - p_{n+1})\,x_{n-1}^{2n-3} = 0 \end{aligned}$$

from which it follows that the co-efficients of ordinates equidistant from the middle are equal.

The first, third, fifth, &c., of equations (4) now become—

$$\left.\begin{aligned} p_1 + p_2 + \ldots\ldots + p_{n-1} + \tfrac{1}{2}p_n &= 1 \\ p_1 x_1^2 + p_2 x_2^2 + \ldots + p_{n-1} x_{n-1}^2 &= \tfrac{1}{3} \\ \ldots\ldots\ldots\ldots\ldots\ldots & \\ p_1 x_1^{2n-2} + p_2 x_2^{2n-2} + \ldots + p_{n-1} x_{n-1}^{2n-2} &= \frac{1}{2n-1} \end{aligned}\right\} \begin{matrix} (a) \\ (b) \\ \\ \end{matrix} \qquad (5)$$

This group (5) series of equations gives an easier form for calculation; and, for illustration, take the Simpson rule for three ordinates.

The multipliers are p_1, p_2, p_3, and the positions x_1, x_2, x_3; as above—

$$x_1 = -x_3 = 1, \qquad x_2 = 0, \qquad p_1 = p_3.$$

From equation (5a) $\qquad 2p_1 + p_2 = 2,$

From equation (5b) $\qquad 2p_1 = \tfrac{2}{3},$

whence $p_1 = \tfrac{1}{3}$, $p_2 = \tfrac{4}{3}$, and the rule is $\tfrac{1}{3} \cdot \tfrac{4}{3} \cdot \tfrac{1}{3}$.

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In this simple case the three multipliers are the only three unknowns, and the curve so assumed to coincide with the given curve is therefore of the second degree.

In the case of Tchebyscheff's rule, the condition is that the multipliers are all equal. This fulfils one condition; there are $(2n-1)$ remaining conditions, which must give the positions of $(2n-1)$ ordinates. It will be found here as before that the ordinates are distributed symmetrically on either side of the middle point, and that the co-efficients are equal in pairs.

In Tchebyscheff's Rule group (4) reduces to

$$\left.\begin{aligned} (2n-1)\,p &= 2. \\ 2x_1^2 + 2x_2^2 + \;.\;.\;.\;.\; + x_{n-1}^2 &= \frac{2}{3p} = \frac{2n-1}{3} \\ 2x_1^4 + 2x_2^4 + \;.\;.\;.\;.\; + x_{n-1}^4 &= \frac{2n-1}{5} \\ &\text{\&c., \&c.,} \end{aligned}\right\} \qquad (6)$$

whence x_1, x_2, &c., can be determined.

The last case taken is that in which the number of ordinates is the same as the number of multipliers, so that the actual number of ordinates used is as small as possible, there being n of each for the general function taken. Here again are found the same conditions as to symmetry of distribution of ordinates and equality of multipliers as before. In the case in which n is an odd number, say, $2m+1$, equations (4) become

$$\left.\begin{aligned} p_1 + p_2 + \;.\;.\;.\;.\; + p_m + \tfrac{1}{2}\,p_{m+1} &= 1 \\ p_1 x_1^2 + p_2 x_2^2 + \;.\;.\; + p_m x_m^2 &= \frac{1}{3} \\ .\;. & \\ p_1 x_1^{4m} + p_2 x_2^{4m} + \;.\;.\; + p_m x_m^{4m} &= \frac{1}{4m+1} \end{aligned}\right\} \qquad (7)$$

The solutions of the equations for various rules are given at the end of the paper. For the Gauss five-ordinate rule, an approximation as regards the multipliers is given, which has been found to involve no appreciable error in the integration, and it is this approximation which is used in the examples of calculation given.

APPENDIX II.

Shape of Functions used in Calculations.

Water-plane Calculations.—It is generally required to know the area, the longitudinal position of the centre of gravity, the transverse and the longitudinal moment of inertia about the centre of gravity. The waterline is a "smooth" curve, and may be represented by a function of the general type stated. It remains to be seen whether, in the various calculations detailed above, there is any probability of introducing any serious change of function or change of smoothness.

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Let y be the ordinate x the distance from mid-length, and let the function be represented by

$$y = \mathrm{F}(x^{2n-1}).$$

then

$$\text{Area of water plane} \quad \ldots \quad \ldots \quad \ldots \quad \ldots = \int y\,dx = \int \mathrm{F}(x^{2n-1})\,dx.$$

$$\text{Moment of area of water plane} \quad \ldots \quad \ldots \quad \ldots = \int y\,dx.x = \int \mathrm{F}(x^{2n-1})\,dx.x = \int \mathrm{F}(x^{2n})\,dx.$$

$$\text{Longitudinal moment of inertia of water plane} = \int y\,dx.x^2 = \int \mathrm{F}(x^{2n+1})\,dx.$$

$$\text{Transverse moment of inertia} \quad \ldots \quad \ldots \quad \ldots = \int y\,dx\,\frac{y^2}{3} = \frac{1}{3}\int \left[\mathrm{F}(x^{2n-1})\right]^3 dx = \frac{1}{3}\int \mathrm{F}(x^{3(2n-1)})\,dx.$$

Comparing these calculations, it is seen that the degree of the functions assumed for moment and longitudinal moment of inertia calculations, is only one and two degrees higher respectively than that of the original function, and that consequently, the rule which integrates one should approximately hold for all three. The actual shapes of these functions are shown in Fig. 1 (Plate XXVII.), from which it will be seen that there is no point of inflection in the moment function, and a slight point in the longitudinal moment of inertia curve.

As regards the function for transverse inertia, its degree is three times the degree of the original function, and it would be reasonable to assume a considerable difference of shape. This is at once seen by inspection of the figure, which is not drawn to the same scale; if it were so, then, supposing the maximum ordinate of the waterline were represented by 20, the maximum ordinate of the inertia function would be 8,000, and the effect of the sudden change would be much more apparent. The need to use more ordinates for the integration of this function was found in actual practice, and the means of dealing with the question are given in the body of the paper.

Displacement and Centre of Buoyancy Calculations.—The displacement is found in two ways, firstly, by finding the area of various water planes, and then integrating the curve of waterline areas with respect to depth; and, secondly, by finding the areas of various transverse sections, and then integrating the curve of area of sections with respect to length.

The longitudinal position of the centre of buoyancy is found by integrating the curve of moments of areas of section with respect to mid-length, and for the vertical position, by integrating the curve of moments of water planes below the load or top waterline.

The actual shape of the functions used in these calculations is given in Fig. 2, from which it will be seen that the curves are free from points of inflection; therefore the shortened rules for integration should be applicable, as was actually found to be the case.

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DISCUSSION.

Professor J. J. WELCH, M.Sc. (Member): Mr. Chairman and Gentlemen, I am sorry to say I have not yet had time to read this paper very carefully. It shows considerable ingenuity, but with regard to the use of Gauss' ordinates in ship calculations, I am bound to say that, without considerable checking of the results, I should be very chary of using only five ordinates in the case of a ship of, say, 400 ft. in length. Of course, if the curve were exactly a curve of the ninth order in the case of five ordinates, being geometrically correct, then we should get correct results, as Mr. Abell states; but I am very doubtful whether actual ship curves could be expressed with such mathematical exactitude, and I should certainly prefer to pin my faith to more ordinates until we had very extended applications of this rule to show that we could use five ordinates only. Of course, "the proof of the pudding is in the eating," and Mr. Abell has pointed out that in the cases to which he has applied the rule, it gives fairly accurate results. The same observation applies to the second part of this paper—viz., the construction of an approximate metacentric diagram. I am sure all would welcome any approximate method which gave promise of accurate results. As Mr. Abell points out, one of the things we want to know at a very early stage is the trend of the curve of metacentres and of the curve of buoyancy; but, here again, it seems to me that very extended investigation must be necessary in order to fix with sufficient accuracy the value of K, which is given here as fitting many types of warships, and in this case also one can only judge of the value of this method by applying it to actual results, although good results can doubtless be obtained for a new vessel of normal type when the average K for that type has been ascertained from earlier examples.

Mr. W. J. LUKE (Member of Council): Mr. Chairman and Gentlemen, I have read this paper with very great satisfaction, and I have had the opportunity of privately complimenting Mr. Abell upon it, and of having a preliminary discussion with him. The subject of curvature is one that I was many years ago closely directed to by Mr. W. E. Smith. He then referred me to a paper on the subject by the late Mr. Charles Merrifield (who was associated with the School of Naval Architecture at South Kensington), which will be found in the British Association Report for 1880. In preparing myself to speak upon this paper, I turned to Merrifield's paper again, and, rather to my surprise (for I had entirely forgotten it), I found Gauss' system there dealt with, though not exhaustively. Mr. Merrifield went at great length into the subject of approximate integration such as we are dealing with here, and anyone who feels disposed to turn to that paper, will find that he sounds a note of warning as to the danger of using too small a number of ordinates. When Professor Biles's paper was read in the Institution of Engineers in Scotland nine years ago on the application of Tchebyscheff's system, that point was brought out there also. As Merrifield says in his paper, while it is quite true that you may correctly integrate a known curve by the use of a few ordinates, the danger of so doing consists in assuming that the practical curve coincides sufficiently closely with the analytical curve to which the selected rule exactly applies. Now, Mr. Abell has applied the method to curves which are tolerably fair, and such as would be found in vessels of fine form. But suppose it to be applied in a case which is common enough in the Mercantile Marine; suppose you take a ship with 50 per cent. of parallel middle body, the curve of areas of sections made up with respect to each quarter-length at the end of a sine-curve ranging from 0° to 90°, you will find that will give a fulness of line of ·82, very nearly. That is not an exaggerated case. If this curve be integrated with four applications of Tchebyscheff's rule for two ordinates, using only eight ordinates in the whole length, a result will be obtained which for all practical purposes is correct; but if Tchebyscheff's rule for nine ordinates be adopted, a result will be got which is quite 1 per cent. in error; and that, not by plotting down the curve and measuring ordinates, but by calculating the individual ordinates. In applying any of these rules of quadrature to practical curves, one inevitably has to assume that the analytical curve embodied in the rule selected for use is sufficiently close to the practical curve. With this

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point in view, I am of opinion that in dealing with types of curves which are not fair in the mathematical sense, it is preferable to use continued applications of an elementary rule, rather than fewer applications of a rule of higher order, as by this means the extent of error in application is reduced to its narrowest limits.

Then, again, we welcomed the use of Tchebyscheff's method as compared with Simpson's method owing to the very considerable saving in arithmetical labour thus made possible, which saving arises almost entirely from the fact that separate multipliers for individual ordinates are not necessary, whereas Gauss' rule involves the use of separate multipliers for separate ordinates, which it is very desirable to avoid, if at all possible. Hence, although we are all indebted to Mr. Abell for having again introduced this subject to our notice, I am inclined to the opinion that Gauss will never get anything like the holding that Tchebyscheff has obtained in work involving practical quadrature. When we go on to the construction of the metacentric diagram, I think Mr. Abell has struck a very good point when he gives us an approximate rule for finding the moment of inertia of a water plane. There is one disheartening thing about it, and it is this: If you refer to the last line on page 247, he says: "The material for merchant ships was not available, so the constants in the approximate relation could not be determined." I am very sorry to find that Mr. Abell has so few friends. I may say that I tried it in a few cases, and found that $k = \cdot 17a + \cdot 13$, which, for a first shot, I thought was a very satisfactory application. These cases have reference to merchant vessels chosen quite by chance, and I suppose they are of much fuller form than those which have been examined by Mr. Abell. There is one other point I would like to mention. In the middle of page 249 there is a paragraph beginning: "While on the subject of the metacentric diagram." Mr. Abell there makes a statement that the metacentre is in the water line when the metacentric curve is horizontal. That must only be taken, of course, as an approximation. The well-known mathematical principle is true, that where the metacentric curve is horizontal, the metacentre and the centre of curvature of flotation coincide. The value of the radius of curvature of flotation is given by the expression $\frac{d\,\mathrm{I}}{d\,\mathrm{V}}$. In one case I have examined, the metacentre was almost exactly at the water line when at its lowest point, but in another case it was 2 ft. above it. Hence, off hand, one is disposed to consider that the approximation given is a rather coarse one.

Mr. JOHN SMITH (Member): Mr. Chairman and Gentlemen, I am afraid that after the learned and exhaustive discourses we have listened to from Professor Welch and from Mr. Luke little remains to be said on this paper. In fact, most of the points I intended to mention have been anticipated, as, for example, the question of the horizontality of the metacentric curve. As Mr. Luke has pointed out, the metacentric curve will be horizontal where the metacentre and the centre of curvature of the curve of flotation coincide. If that happens to agree with the point where the metacentric curve crosses the 45° line, then what it means is that the metacentre and the centre of curvature of the curve of flotation are both in the waterlpane, that is to say, the radius of the curve of flotation is zero; and then from Leclert's theorem $\rho = \frac{d\,\mathrm{I}}{d\,\mathrm{V}}$ it follows that for very slight changes of draught the moment of inertia of the water line is constant. This is not a new point, but perhaps the method of stating it is better than the proof given by Mr. Abell, as it shows up better the nature of the conditions which must hold.

With regard to the application of the Gauss rule to the integration of ship curves, it may be said that so far it has been received with caution, and I think it only right that this should be the case; but from the theoretical point of view, I think it is very interesting. Most of us thought that after the development of the Tchebyscheff rules and their application to ship integration, there remained practically nothing to investigate in connection with this subject. You may take a ship curve (and, as Mr. Abell points out, all calculations eventually reduce themselves to finding the area of a ship curve of some sort), and you may divide it by a large number of equally spaced ordinates, and by applying Simpson's rules you get simple multipliers applied to those ordinates to get the area of the curve. Here the spaces are all unity and the multipliers simple whole numbers. Tchebyscheff wanted to

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simplify the process of integration at the other end of the scale by assuming that all the multipliers were unity, and finding what the corresponding spacing should be; and the spacings in his rules work out very unevenly. Between these two limits there are quite an endless number of rules that may be formed according to the assumptions made. You may arbitrarily assign the spacing and calculate what the multipliers should be, or you may arbitrarily assign the multipliers and calculate what the spacing of the ordinates should be to agree with those multipliers. Professor Burnside and Mr. Abell sought a solution in another direction, and tried to find a simple rule; that is to say, to find what would be the smallest number of ordinates with which a good result could be obtained, regardless of evenness of spacing or simplicity of multipliers; and I think that was a search that was very well justified. The fact that is evolved from the investigation is that the spacing is very irregular, and that the true multipliers are very formidable to deal with; but I think that Mr. Abell deserves the thanks of the Institution, and of students generally, for finding an approximation, a sort of levelling up of the multipliers by getting rid of the cumbrous true multipliers and substituting the simpler ones 5, 10 and 12.

I think it ought to be pointed out that the danger of applying a rule of this sort comes in more particularly in connection with moments and with moments of inertia. It is quite impossible to find the mathematical equation of a ship curve—it could not be done; so that all that these methods of quadrature finally come to would be this, that for the actual ship curve you substitute some mathematical curve, coinciding with the actual ship curve at any prearranged number of points, and then find the area, &c., of the substituted curve. The curve generally taken is parabola of a more or less formidable degree, and in this present case a ninth degree parabola is taken. It is easy to choose such a parabola passing through five points on the ship curve, and such that the area inside the parabola is very nearly equal to the area of the ship curve; but that must be done—since the curves cannot coincide at all points—by the removal of portions of area from some points and their transfer to others, otherwise the two areas will not agree. That necessarily involves a disagreement in the moments of the two curves, and they will disagree still further as regards the moments of inertia, and the fewer the number of points of coincidence the greater is the probability of error; so that it is in the application of these rules to moments and moments of inertia that the caution exhibited by the previous speakers should be displayed, rather than in their application to areas.

Mr. W. H. Whiting (Member of Council): Mr. Chairman and Gentlemen, I am very sorry that my official work prevented me from hearing the early part of the discussion on what is a very interesting paper from several points of view. Now this is not the first time, by any means, that this subject has been before us. It seems a simple thing to find the area of plane surfaces bounded by curves of a not very elaborate character; yet, in the very earliest numbers of our Transactions you will find papers on the subject. Even before the establishment of this Institution, mathematicians had often been engaged in devising methods of integrating areas bounded by simple curves of the sort we have to deal with; and the practical application of these methods has been a matter of continual study, yet, often as the field has been gone over, there are still, as this paper shows, some ideas of value to be gleaned by those who search carefully, as Mr. Abell has done. In regard to the direct practical value of the method investigated in this paper, I am reminded of what I have heard Sir William White say, that after you have investigated and invented these somewhat elaborate rules, you nearly always go back in your general work to Simpson's ordinary rule. That is my own experience, and there is a good reason for it. Simpson's rule, if well used, is singularly well fitted for the integration of ship-like areas. It is very accurate; it is easy to apply, so as to bring almost the whole of the work into the one operation; and you can vary its application from point to point so as to maintain reasonable uniformity of error. I notice in Mr. Abell's application of the rule investigated in this paper that although he first of all gets rid of the greater part of the more difficult work by cutting off a very large lower appendage; that is to say, although he restricts the application of Gauss' and Tchebyscheff's rules to the simplest portion of the hull, he still finds it expedient, as he says in the body of the paper, to use Simpson's multipliers for one integration. The fact is, Simpson's ordinary rule is a rule of

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extraordinary power and flexibility; and it has also got, as has been shown, a very high degree of accuracy, if you apply it in the way in which its undoubted flexibility permits. But this in no way diminishes the interest of the method which Mr. Abell has so clearly investigated, and I desire to express my appreciation of the paper, and also the hope that we shall receive further contributions from him to the Transactions of the Institution.

Mr. W. S. ABELL, R.C.N.C.: Mr. Chairman and Gentlemen, I have to thank you for the kindly manner in which the paper has been received. It is a point of some interest, as Mr. Smith stated, to find out the minimum number of ordinates which would integrate a given function, and that point is, I think, adequately dealt with in the paper; and there is no danger in using a minimum number of ordinates, provided the conditions of use are properly understood. The length does not affect the question, as all integration rules give the mean ordinate of the curve, irrespective of length. It is stated on the first page of the paper that "the area cannot be approximately calculated by a function passing through the point where this sudden change of slope occurs." Perhaps this point was not sufficiently emphasised; but in the case Mr. Luke mentions, the effect of putting in a parallel middle body is obviously to introduce discontinuity at the ends of the middle portions and any rule involving a continuous function could not be expected to give a good result in such a case. Simpson's, Gauss', or Tchebyscheff's rules would, however, all integrate the end portions, and a composite rule could, no doubt, be found which would apply. I might say that probably it would not be safe to apply any method involving continuous functions to ships having a higher block co-efficient than ·65, or having any parallel middle body. As regards the point that multipliers lead to an increase in arithmetical labour, less measurements are required in Gauss' rule than in Tchebyscheff's, and, further, the multipliers are very simple, being written, if preferred, as 1, 2, and 2·4, so that but little error is likely to occur. Moreover, in stability work, the error in using multipliers is probably less than that involved in tracing over four more sections of the ship. As regards the values of the constants m and n in the formula $\frac{K}{B} = m \cdot a + n$, in the case of merchant ships, my point was that the information I had was not in a desirable form. In the case of any particular shipbuilding firm, I think it will be found that there is an interconnection between one ship and another, one form being derived from a previous type of ship. It should therefore be comparatively simple to evaluate m and n by taking a series of ships built by the same firm, whereas I had only isolated cases of quite different types, and hence it was not possible to give any value to the constants which could be expressed in so simple a form as that given.

As regards the point about the metacentric curve being horizontal where the curve cuts the 45° line, the assumption used in the paper, that I is constant, makes the centre of the curve of flotation coincide with the metacentre, and, further, shows that the metacentre should be in the water-line. There is a rather better proof, which assumes that K is constant, and which is due to one of the students at Greenwich. Mr. E. F. Spanner, who also found a better approximate value for the lowest point of the metacentre. It is as follows, using the notation of the paper:—

$$BM = \frac{I}{V} = \frac{a}{b}\frac{K^2}{D},$$

whence, assuming K is constant, $BM \times D = \text{Constant}$. (4)

Differentiating $\delta BM \cdot D + BM \cdot \delta D = 0$,

or $\delta BM = -\frac{BM}{D} \cdot \delta D$.

The rise of the centre of buoyancy $= \delta D \tan \theta$,

$= \delta D \times \frac{A\,h}{V}$,

$= \delta D \times \frac{a}{b} \cdot \frac{h}{D}$.

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Summing the two effects, the rise of the metacentre with increase of draught is

$$-\frac{\mathrm{B\,M}}{\mathrm{D}}\,\delta\,\mathrm{D}+\frac{a}{b}\cdot\frac{h}{\mathrm{D}}\,\delta\,\mathrm{D}$$

$$=\frac{\delta\,\mathrm{D}}{\mathrm{D}}\left(\frac{a}{b}\cdot h-\mathrm{B\,M}\right), \qquad (6)$$

instead of $\frac{\delta\,\Delta}{\Delta}(h-\mathrm{B\,M})$ as given in equation (5) of the paper.

It follows from (6) that the metacentric curve is horizontal when $\mathrm{B\,M}=\frac{a}{b}\cdot h$, and, as $\frac{a}{b}$ is greater than unity, B M must be greater than h, or the metacentre is above water by the amount $\left(\frac{a-b}{b}\right)h$. The result shows that the lowest point of the metacentre occurs before the curve cuts the 45° line, which agrees with the observed facts better than my formula.

As regards the point made by Mr. Smith about moments and moments of inertia, there is certainly something to be said as to the probability of an error occurring with the longitudinal moment of inertia, but I do not think it is of any great extent. There is not much to be feared if the point of view is taken that the moment involves a curve only one degree higher than the original curve, for which case it can generally be assumed that the rule holds good. If n ordinates are taken and the same rule used, it probably integrates $(n+1)$ ordinates with the same degree of accuracy, which point is covered by the fact that it is not advisable to use an even degree rule. It is not easy to show, but Professor Burnside has told me that it is probably true that an n ordinate rule also integrates $(n+1)$ ordinates, with about equal accuracy.

Mr. John Smith: That is not quite my point. My point was: You have only five points of coincidence between the actual ship curve and the assumed parabola, whatever it may be. You can get a parabola to agree with the ship curve as regards a given area by cutting under it in one place and over it in another; therefore, you must affect the moment and also the moment of inertia. It is not a question of integrating a mathematical curve of higher degree with a rule applied to a lower degree.

Mr. W. S. Abell: It is not easy to say exactly how the assumed function and the given curve agree, but, although only five ordinates are taken, yet the conditions fulfilled are such that the given curve and the assumed function coincide in nine and not five points only. In the cases I worked out, I found no indication of any greater error than that stated—viz., one-half per cent.—in the case of moment or moment of inertia evaluation.

The Chairman (Professor J. H. Biles, LL.D., Vice-President): Gentlemen, I am sure you will allow me to convey our thanks and the thanks of the Institution to Mr. Abell for his very interesting paper. The subject, as Mr. Whiting has said, is perennial, and no doubt Macaulay's New Zealander will find somebody in the Institution of Naval Architects reading papers on quadrature when he comes here to take notes. The rule that Mr. Abell has put forward is good for the purpose that he intends it to fulfil. With the small number of ordinates, you get a fair degree of accuracy—the same kind of accuracy that you get with a larger number of ordinates in some other rules. In this connection I am sorry that there has not been more work done in relation to standardising results of ship's calculations. Some years ago, I brought to the notice of the Institution a method by which we could all bring the results of calculations to a common basis, regardless of size of ship. If we had done that more frequently, and compared notes, I think Mr. Abell would not have been in the position of lacking data for merchant ship forms that he appears to be at the present moment. If he will look at that paper of mine, he will find a great deal of information given there on all kinds of forms—merchant ship forms, warship forms, and yacht forms, and I think he may get some of the information he wants from that paper. I ask you to pass a hearty vote of thanks to Mr. Abell for his very interesting paper.

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FACTORS OF SAFETY IN MARINE ENGINEERING.

By Professor JOHN OLIVER ARNOLD, of Sheffield University.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 10, 1908; the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D., in the Chair.]

ON the vital importance of the subject dealt with in this paper it is obviously unnecessary to dilate, and in dealing with it the author will endeavour to sustain the somewhat difficult, delicate, but, unfortunately, necessary part of acting as a sort of connecting link between engineers and metallurgists. He fully expects some adverse criticism from the ranks of the first-named, since it will be his duty to deny the truth of dangerous dogmas which have to a considerable extent, in connection with engineering calculations, held the field since 1871.

In discussion upon matters such as those dealt with in this paper, engineers have occasionally insisted on the great importance of basing all calculations on the elastic limits of the materials used in construction; but, speaking generally, such contributors to debate have been somewhat akin to finger-posts, which point out the way but never travel by it. Broadly speaking, engineers always base their calculations on the maximum stress (often absurdly called the "breaking strain")* capable of being endured by the metal employed. The reasons for this procedure are:—

(1) The maximum stress is a value capable of accurate and rapid measurement on the static machines used in ordinary works practice.

(2) It is more or less an accepted canon in engineering faith that the maximum stress is really an indirect measurement of the elastic limit in the ratio of about two to one. There is no doubt that in the majority of cases the rule just specified is approximately true, but it is also equally true that in a relatively small, but nevertheless extremely important number of instances, this widely accepted datum is hopelessly inaccurate. So far as the author has been able to ascertain, factors of safety in marine and other engineering practice are calculated as follows:—

Firstly.—In a rough-and-ready fashion by taking care to subject the material of construction only to a working stress varying according to circumstances from about

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* The "breaking strain" is the total elongation in inches, and yet one often sees report sheets expressing this linear dimension in tons per square inch.

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one-quarter to, say, one-tenth of the maximum stress capable of being endured by the material.

Secondly.—In scientific engineering a more elaborate course of procedure obtains. Let 1 = the maximum stress of the material and n = the factor of safety, then $\frac{1}{n}$ = working load. The factor n is obtained as a product of several sub-factors, which may be designated as a, b, c, and d. Sub-factor a is the ratio of the elastic limit to the maximum stress, and is almost universally, in connection with mild structural steel, assumed to equal $1\frac{1}{2}$ to $2\frac{1}{2}$. Sub-factor b is a variable, dependent on the character of the stress to which the material is subjected, and, broadly speaking, is based on the classical work published by Wöhler in 1871, which indicated that metals subjected to rapidly varying stresses, will ultimately break under a load within the elastic limit.

TABLE I.*

SHOWING DERIVATION OF FACTORS OF SAFETY FROM THE COMPONENT SUB-FACTORS.

Type of Purpose for which the Steel is to be used.	Nature of Sub-factors Involved.				$n = a \times b \times c \times d =$ Factor of Safety.
	a (Elasticity).	b (Wöhler Phenomenon).	c (Rapidity of Load).	d (Unknown Contingencies).	
Boilers	2	1	1	$2\frac{1}{2}$—3	4·5—6
Double-acting connecting rod ...	1·5—2	3	2	1·5	13·5—18
Single-acting connecting rod ...	1·5—2	2	2	1·5	9—12
Shaft carrying propeller ...	1·5—2	3	1	1·5	6·75—9
Steel cast-wheel-rim	2	1	1	4	8
Steel castings	2	1	1	2	4
Oil tempered steel	1·5	1	1	1·5	2·25
Nickel steel	1·5	1	1	1·5	2·25

* The figures in this Table were kindly compiled for the author by Mr. J. W. Kershaw, M.Sc., of the Engineering Department of Sheffield University.

Taking three typical cases, we have :—

(1) A steady load, as in bridges and boilers, in which $b = 1$.

(2) Loads varying between zero and a maximum as in single-acting connecting rods, where $b = 2$.

(3) Alternating loads, where stress varies from tension to compression, as in a double-acting connecting rod, where $b = 3$.

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Sub-factor c is a variable dependent upon the rapidity with which the stress is applied. A sudden load is supposed to stress the material to twice the extent of one gradually applied, therefore $c = 1$ for a steady load, 2 for suddenly applied load, and, say, 3 for load including impact. Sub-factor d is used as a margin for unknown conditions and contingencies, and is a variable ranging from $1\frac{1}{2}$ to 3. Table I. embodies concrete examples of the application of the foregoing sub-factors.

In 1895 the author read before the Institution of Civil Engineers a paper on "The Influence of Carbon on Iron,"* which engineers have apparently considered of small practical value. After an interval of a dozen years the author again ventures to call the attention of marine engineers in particular to the remarkable statical test data published in that paper, and also to show that some of the sufficiently disconcerting mechanical facts embodied therein, were considerably understated. In connection with the main points at issue, namely, the properties of more or less mild steels connected with structural materials of engineering, it is necessary to quote only a fraction of the data just referred to, namely, that concerned with steels ranging in carbon from about 0·1 per cent. to 0·4 per cent. The steels investigated were submitted to tests in two widely differing micro-chemical conditions, produced by two widely diverse rates of cooling. These two conditions may be specified by the terms, "normalised" and "annealed," or, to be strictly accurate, "annealed to death." The tabulated results of these particular steels are now reproduced from the Proceedings of the Institution of Civil Engineers in Tables II. and III. of the present paper. The requisite thermal definitions are as follow :—

The normalised steels were heated to a bright red or orange, say, 950° C., and were spontaneously cooled in air. The annealed samples were maintained at a similar temperature for about 72 hours, and were then allowed to cool to milk warm during an additional 100 hours.

TABLE II.

NORMALISED STEELS.

Steel No.	Carbon per cent.	Elastic Limit. Tons per square inch.	Maximum Stress. Tons per square inch.	Elongation per cent. on 2 in.	Reduction of Area per cent.
1	0·08	12·19	21·39	46·6	74·8
$1\frac{1}{2}$	0·21	17·08	25·39	42·1	67·8
2	0·38	17·95	29·94	34·5	56·3

* Min. Proc. Inst. C.E., Vol. CXXIII. p. 127.

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TABLE III.

ANNEALED STEELS.

Steel. No.	Carbon per cent.	Elastic Limit. Tons per square inch.	Maximum Stress. Tons per square inch.	Elongation per cent. on 2 in.	Reduction of Area per cent.
1	0·08	8·82	18·34	52·7	76·7
1½	0·21	9·02	21·25	42·3	65·7
2	0·38	9·55	25·02	35·0	50·6

In connection with factors of safety the important columns in the foregoing tables are those originally alleged by the author to be the elastic limits of the three steels in the normal and annealed conditions, but it must now be confessed that the columns headed "elastic limit" should have been headed "yield point, or apparent elastic limit." The author's subsequent experience has shown that the difference between the yield point and the true elastic limit in normal structural steels is about 1¾ and in the over-annealed steels about 3½ tons per square inch. In view of these facts, the approximately correct figures are shown in the following table :—

TABLE IV.

Steel No.	Carbon per cent.	Condition of Steel.	Approximate True Elastic Limit. Tons per square inch.
1	0·08	Normalised	10·44
1½	0·21	do.	15·33
2	0·38	do.	16·20
1	0·08	Annealed	5·32
1½	0·21	do.	5·52
2	0·38	do.	6·05

The maximum stress curves of the normal and the elastic limit curves of the normal and annealed steels are plotted in Fig. 3 (Plate XXIX.), and will be dealt with later.

As bearing upon the results last tabulated, having reference to forged, and forged and annealed steels, it will be well to call attention to a result registered by an annealed steel casting, and published by the author about ten years ago during an extension lecture given at the East End of Sheffield. This result, which was received

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with profound scepticism (in which the author was at the time inclined to share), was as follows:—

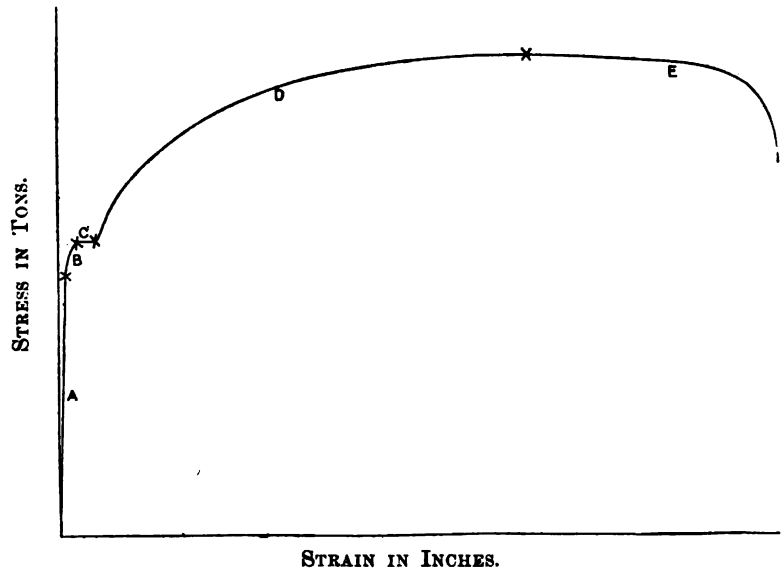
TABLE V.

Condition of Steel.	Elastic Limit. Tons per square inch.	Maximum Stress. Tons per square inch.	Elongation per cent. on 2 in.	Reduction of Area per cent.	Bending Test.
As cast	17·6	25·5	5·0	5·6	21° broken
Annealed	7·4	24·3	32·5	37·7	180° unbroken

This casting contained 0·4 per cent. carbon, 0·26 per cent. silicon, 0·10 per cent. manganese, together with low sulphur and phosphorus. The figures 7·4 have reference to the yield point, *hence the true elastic limit of this material was really approximately* 3·9 *tons per square inch.* An elastic limit so astoundingly low as about 4 tons per square inch justifies the author in putting forward as a tentative hypothesis, that mild steel by over-annealing may actually (like pure copper) have practically *no elastic limit*, that is to say, at any point above the zero of a stress-strain diagram the metal may assume plastic, as distinct from elastic, deformation. In such a case, the factor of safety must be, theoretically, almost infinitely great to prevent the ultimate fracture of the steel. An ordinary stress-strain diagram of steel, the co-ordinates of which are, of course, stress in tons and strain in inches, may be divided into five ranges (see Fig. 1). Range A is a very steep straight line, along which the material is assumed to be perfectly elastic, and within which equal increments of stress produce equal increments of strain, such strain being essentially elastic deformation. This line has been named the "range of proportionality," and its upper end is supposed to mark the true elastic limit of the metal under test.

FIG. 1.

MILD STEEL STRESS-STRAIN DIAGRAM.—TYPE I.



Range B is a short and variable curve the beginning of which touches the range of proportionality, and the end merges into range C.

Range C is often a short, nearly horizontal, line marking the yield point or apparent elastic limit.

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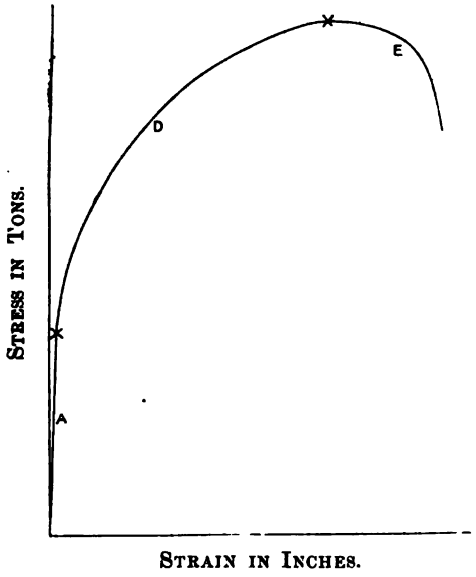
Range D is a variable curve, the highest point of which marks the maximum stress.

Range E is a falling curve, the lowest point of which measures vertically the breaking stress, and horizontally the breaking strain, or total elongation in inches; but in connection with drastically annealed structural steels, the second type of curve (see Fig. 2) is often registered, which has only three of the ranges observed in the first diagram: namely, range A an alleged range of proportionality; D, a flowing curve extending from the end of the supposed straight line to the point of maximum stress; E, a curve beginning at the point of maximum stress and ending at the breaking stress and maximum strain point.

FIG. 2.
MILD STEEL STRESS-STRAIN DIAGRAM.—TYPE II.



With reference to the type of curve exemplified in Fig. 2, in view of the enormously disproportionate dimensions of the measured co-ordinates of a stress-strain diagram, namely, stress in tons per square inch and strain in, say, $\frac{1}{10,000}$ in. it is hardly an unreasonable question to ask, is range A really a proportional line, may it not be a curve from zero? or contingently, to vary the question, if this be so, where is the elastic limit, if any, of the material?

Referring to Fig. 3 (Plate XXIX.) it will be noted:—

(1) That the observed maximum stress curve shows, in normal mild steels almost free from manganese, as the carbon falls from about 0·4 to 0·1 per cent., that, with a constant factor of safety of 1 in 6, the working stress falls from 5 to 3·75 tons per square inch.

(2) That the approximately observed elastic limit curve shows that in normal steels the assumed factor of safety of 3 is really exceeded, ranging from 3·06 to 3·63.

(3) That, assuming an engineer had calculated his factors of safety on a basis of 6 to 1 on the maximum stress, involving a factor of safety of at least 3 to 1, on the elastic limit of the normal, then, if previous to delivery the steel had been (with the best intentions) over-annealed, the approximate elastic limit, instead of averaging 3 or upwards, will average less than $1\frac{1}{2}$.

The absurdity of the foregoing hypothesis is mitigated only by the fact that, within the author's knowledge, such cases have occurred in actual practice. The curves plotted in Fig. 3 (Plate XXIX.) probably represent in concrete tonnage the extreme possibilities of over-annealing, because of the practical absence of manganese from the series dealt with. In practice, open-hearth steels generally contain from 0·4 to 0·8 per cent. of this element, which considerably modifies the action of over-annealing in its

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excessive lowering of the elastic limit. Therefore the author advocates the percentage last named.

The startling micrographic changes brought about by over-annealing structural steels are exhibited at their maximum (again owing to the low manganese present) in Fig. 4 (Plate XXVIII.). It has reference to the steel containing 0·38 per cent. of carbon. The left half micrograph shows a normalised steel, consisting of a white sectional ground mass of ferrite or iron (lightly etched, so as not to develop the junction lines of its allotrimorphic crystals) overlaid with dark etching normal pearlite, consisting of 87 per cent. iron, through which is evenly distributed in ill-formed laminæ 13 per cent. of carbide of iron Fe_3C. The right-hand half section shows the over-annealed steel in which the carbide Fe_3C has sharply segregated, passing partly into parallel lines characteristic of Sorby's laminated pearlite and partly into masses marking the commencing decomposition of laminated pearlite into cementite (B carbide) and ferrite. The texture of the over-annealed metal is much coarser than that of the normal steel. The author has found that, roughly speaking, the true elastic limit of pure normal pearlite is about 23 tons per square inch, whilst that of laminated pearlite is only about 13 tons per square inch. He has also observed that the true elastic limit of pure normal ferrite is about 10 tons per square inch, whilst the true elastic limit of over-annealed ferrite is about 5 tons per square inch. For manganiferous pearlite and ferrite the foregoing figures are considerably raised.

Failure of Factor of Safety Calculations in Actual Practice.

In describing such failures it would obviously be improper for the author to specify particular cases. He therefore gives a typical example, which is really the mean result of several cases he has been from time to time commissioned to investigate. This average case includes about 20 careful determinations each, of the maximum stress and elastic limit of the steels involved, which were for the most part in the form of hollow shafts, ranging between 22 in. and 25 in. in diameter. The main static results of the "pooled" tests are embodied in Table VI.

TABLE VI.

True Elastic Limit. Tons per square inch.	Maximum Stress. Tons per square inch.	Elongation per cent. on 2 in.	Reduction of area per cent.
8·5	28·5	30·5	52·5

All the shafts involved had evidently been over-annealed, not necessarily in the sense that they had been over-heated, but rather that they had been kept for too long a time in a low red range of temperature. It is quite probable that in designing these shafts the material was supposed to have a maximum stress of 30 tons per square

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inch and an elastic limit of at least 15 tons per square inch, and that the factor of safety was to be, say, 7 to 1, this is equivalent to a working stress of about 4·3 tons per square inch. The intention and actual achievement of our abstract designer are embodied in Table VII.

TABLE VIII.

Factor of safety calculated on assumed maximum stress.	Factor of safety calculated on actual maximum stress.	Factor of safety calculated on assumed elastic limit.	Factor of safety calculated on actual elastic limit.
7·0	6·65	3·5	1·9
Adequate	Adequate.	Adequate.	Quite inadequate.

The shafts, of which the above mean table is typical, all fractured in use, and the result in the last compartment of the table distinctly understates the worst case involved therein.

On Ghost Lines.

Annealed large shafts nearly always exhibit decarbonised ghosts. In order to avoid scientific confusion, and any appearance of encroaching on the domain of an eminent scientist, it may be well at once to state that "ghosts" of steel are invariably materialised and dangerous, and in the author's experience (which, however, is quite subject to revision) the sequence of their materialisation is as follows:—

In a big ingot, irrespective of the liquated and scrapped upper third, in parts of which the phosphorus, for instance, may exceed 1 per cent., there is always more or less a segregation of the mobile elements, carbon, sulphur, and phosphorus, to a series of centres. Investigations on the "Diffusion of Elements in Iron," carried out at the Sheffield University College, and published in the Journal of the Iron and Steel Institute, No. 1, 1899, indicated the mobility of carbon to be about five times as great as that of sulphur, phosphorus, and nickel. The upper micrograph in Fig. 5 (Plate XXVIII.) shows a dark etching nodular segregation area. This consists of iron containing an undue proportion of (*a*) an isomorphous mixture of the double carbides of iron and manganese (x Fe_3C, y Mn_3C); (*b*) dissolved phosphide of iron (Fe_3P); (*c*) small segregated irregular globules of sulphide of manganese (MnS). On forging the ingot the angular or martensitic structure is broken up, whilst the nodular segregate is drawn out into a dark etching rod (see also micrograph Fig. 5, Plate XXVIII.). In such a forging the elongated segregation, which is relatively hard, is in turning operations jumped by the tool, leaving in faint relief a relatively white line, hence the turner's somewhat far-fetched name "ghost." During prolonged cooling at a low red heat, the carbonised ghost becomes decarburised, the dissolved phosphide of iron seeming to expel the hardenite (transformed pearlite) to the edges of the ghost line, the final product being the decarburised ghost

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line, as exemplified in Fig. 6. Here the ghost has become essentially a broken and irregular cylinder of pearlite, filled with pale brown-etching ferrite containing emulsified phosphide of iron, through which is scattered short rods of dove-grey sulphide of manganese.

Dynamic or Kinetic Testing in its Bearing on Factors of Safety.

The pioneer, valuable and extensive researches, published by Wöhler in 1871, having reference to alternating stresses, both distributed and at a maximum on the skin of metals, have deservedly ever since largely influenced engineering design with reference to factors of safety; but it must be remembered that Wöhler was not a twentieth-century steel metallurgist, and that some of his supposed disciples have made and acted upon deductions from his experiments not authorised by more thoughtful confirmers and exponents of his work. For an instance of this, the following extract from an American book on steel may be quoted :—

"There are people even now who would rather have their forgings made of a mild steel which is weak, than of a high carbon steel which is strong, simply because the old ideas are not yet eradicated from their minds. Tests were made at the Government testing bureau at Watertown by rapidly bending bars forward and backward within their elastic limit with the following results, and these have given engineers an idea of the comparative endurance of wrought iron and steel in such service as that to which the crank pins, shafts, &c., are subject."

Tests of Steel under Repeated Stresses.

"Under a fibre stress of 40,000 lbs. per square inch,

Wrought iron breaks after	...			50,000	alternations of stress."			
·15 p.c. carbon steel breaks after				170,000	„	„	„	
·25 p.c.	„	„	„	„	229,000	„	„	„
·35 p.c.	„	„	„	„	317,000	„	„	„
·45 p.c.	„	„	„	„	976,000	„	„	„

To readers who, like the writer of the above extract, are imperfectly acquainted with the researches and calculations of Wöhler and other competent experimenters who have subsequently confirmed them, the information given might mean (if it means anything) that an engineer may replace a wrought-iron crank pin of, say, 19 in. sectional area with a pin of 0·45 carbon steel, only 1 in. in square area, which is absurd.

One of the most complete accounts of Wöhler's experiments, and probably the most thoughtful and judicious commentary thereon is to be found in Professor Unwin's work on "The Testing of Materials of Construction." The term "Fibre Testing" is, however, a complete misnomer, since wrought iron and mild steel are now well known to be crystalline bodies, and the only "fibres" connected with them are, respectively,

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streaks of slag and sulphide of manganese. Perhaps the most valuable of Wöhler's alternating stress tests were carried out in the following manner, shown diagrammatically in Fig. 7 (Plate XXIX.). B is a rotating test bar firmly inserted in a die D, the stress being applied (diagrammatically) by the weight W. The theoretical cycle of stress changes as the bar rotates as shown in the lower diagram of Fig. 7. $M T^s$ is the point of maximum skin tension or + stress, dying away to nothing at Z, then passing into compression stress, reaching a maximum skin compression or − stress at $M C^s$. This − stress dies away to zero at Z′, when the point again passes into + stress, once more reaching a maximum at $M T^s$. The author selects from Wöhler's work three of the sets of experiments quoted by Unwin. The first set has evident reference to wrought iron, having statically an average maximum stress of 21·3 tons per square inch, associated with an average elongation of 19·8 per cent., the figure last named having probable reference to a parallel length of 8 in. The second set is said to have reference to "homogeneous iron," but probably represents mild steel or ingot iron, having a mean maximum stress of 28·2 tons per square inch in association with an elongation of 20·8 per cent. on 8 in. The third set is designated as Krupp's "cast steel axles," registering statically a mean maximum stress of 46·4 tons per square inch, with an elongation probably on 8 in. of 16·7 per cent. This is presumably a crucible steel containing about 0·6 per cent. carbon.

WÖHLER'S EXPERIMENTS ON BARS SUBJECTED TO REPETITIONS OF TRANSVERSE STRESS (ROTATING BARS) BETWEEN EQUAL AND OPPOSITE LIMITS OF STRESS.

SET I.

No. of Bar.	Material.	Stress applied in tons per square inch.		Range of stress in tons per square inch.	No. of repetitions before fracture.
		Maximum.	Minimum.		
1	Iron for axles, Phœnix Co.	+ 15·3	− 15·3	30·6	56,430
2		14·3	14·3	28·6	99,000
3		13·4	13·4	26·8	183,145
4		12·4	12·4	24·8	479,490
5		11·5	11·5	23·0	909,840
6		10·5	10·5	21·0	3,632,588
7		9·6	9·6	19·2	4,917,992
8		8·6	8·6	17·2	19,186,791
9		7·6	7·6	15·2	132,250,000*

* Not broken.

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SET II.

No. of Bar.	Material.	Stress applied in tons per square inch.		Range of stress in tons per square inch.	No. of repetitions before fracture.
		Maximum.	Minimum.		
24	Homogeneous Iron.	+23·9	−23·9	47·8	2,375
25		22·9	22·9	45·8	4,986
26		21·9	21·9	43·8	11,636
27		18·2	18·2	36·4	31,586
28		16·3	16·3	32·6	94,311
29		14·3	14·3	28·6	161,262
30		13·4	13·4	26·8	464,786
31		12·4	12·4	24·8	636,500
32		11·5	11·5	23·0	3,930,150
	SET III.				
33	Krupp's cast steel axles.	+20·1	−20·1	40·2	55,100
34		17·2	17·2	34·4	127,775
35		16·3	16·3	32·6	797,525
36		15·3	15·3	30·6	642,675
37		15·3	15·3	30·6	1,665,580
38		15·3	15·3	30·6	3,114,160
39		14·3	14·3	28·6	4,163,375
40		14·3	14·3	28·6	45,050,640

With reference to the first two experiments in Set I. and to the first three experiments in Set II., it appears almost certain that in these five tests the bars were stressed well beyond their elastic limit and were hence also plastically strained.

Author's Alternating Stress Test.

In July, 1905, Mr. J. T. Milton, Chief Engineer Surveyor to Lloyd's Register of Shipping, read before the Institution of Naval Architects a paper on "Fracture in Large Steel Boiler Plates."* In that paper Mr. Milton quoted from reports made to Lloyd's Committee on the application of a new modification of Wöhler's method

* Trans. I.N.A., Vol. XLVII. p. 359.

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devised by the author for testing some large boiler plates, which had split under the hydraulic test. At that time the method had only just emerged from the experimental stage, but had proved beyond all doubt that a piece of the faulty boiler plate, 12 in. by 6 in. by 1 in., which gave satisfactory static tests, good chemical analyses, and was identical all over in micro-structure, exhibited astounding variations in the resistance of its different parts to alternating stress, the most brittle bar having a resistance to rupture less than one-fifth of that registered by the best test recorded, during trials on 32 bars. The author's original idea in devising this test was to bring Wöhler's method into practical works use by reducing the time occupied in making a test from hours to seconds. The author argued that if the tests, instead of being made (as is general) within the elastic limit, were always made just beyond it, the results would quickly reflect the liability to fracture within the elastic limit, under stresses applied for a very long period. But it was found that the test predicted liability to fracture from causes altogether apart from Wöhler's fatigue phenomenon, and registered potential brittleness, which the Wöhler test not only failed to detect, but emphatically pronounced to be absent. The left-hand sketch of Fig. 8 (Plate XXIX.) shows the nature of the author's tests. B is the test piece, $\frac{3}{8}$ in. diameter and 5 or 6 in. long. It is gripped in the die D, and the stress is applied by the strokes of the slotted plunger P P. The standard conditions adopted for general works practice are:—A rate of alternation of 650 per minute, a distance of 3 in. from the zero of stress (the striking line on the radius of the plunger) and the plane of maximum stress (the top of the die line). The deflection at the zero of stress is $\frac{1}{8}$ in. each side, ensuring a strain just beyond the elastic limit on the plane of maximum stress. At the end of a stroke of the plunger P P in the direction indicated by the arrow, the point M T^{ss} indicates the point of maximum tensional stress and plastic strain, whilst the point M C^{ss} indicates the point under the maximum compressional stress and plastic strain. The right-hand diagram in Fig. 8 shows the alternation of the stress-strain lines in the author's test. In the upper circle, with the stress in the direction of the arrows, the point M C^{ss} is the maximum of compression, and the point M T^{ss} is under the maximum tension stress, the line Z being the fixed zero of stress. In the lower circle, representing the reverse stroke of the plunger, the stresses on the given points have respectively changed from — to + and from + to —. It will be seen that in this test the points of + max. and — max. stress, although alternating, are fixed, as is also the zero line of stress; whereas in the Wöhler bending test a given point passes from + max. to zero, from zero to — max., from — max. to zero, and from zero again to + max., thus completing the cycle. The author's test was frequently found to give unexpected information of considerable value in determining, not to say the homogeneity, which in such connection is an absurd term, but the evenness of the texture of a steel as regards the crystallisation of its ferrite and the distribution of its pearlite and sulphide of manganese. It should, however, be remembered that a steel with unsatisfactory physical properties—in other words, a steel of bad quality—may nevertheless be even in texture, while conversely a steel of uneven texture may,

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as a mass, be of good quality. The circles forming the upper diagrams of Fig. 9 (Plate XXIX.) indicate three types of texture to be observed in mild steels. The direction of the tension stresses and the original points of maximum stress are indicated by the arrow heads. Circle No. 1 represents a fracture of a steel of very even texture, in which fracture has commenced almost simultaneously at exactly opposite points on the skin of the test piece, and proceeded at identical rates inwards, meeting on a line indicating the theoretical zero of stress. The area of metal last to fracture is a strip which breaks in tension under the last strokes of the plunger, leaving a grey line distinct in appearance from the two half-circles constituting the main fracture; thus, as in circle 1, this line is registered in the middle of the bar and at right angles to the direction of stress. In circle 2 is represented the fracture of a bar of very uneven texture, in which rupture had commenced on the right-hand skin considerably before the metal began to break on the left-hand skin, so that the grey line, showing a final tensional fracture, bisects the left-hand semicircle of the bar, but, as in case 1, is still at right angles to the direction of stress. Circle 3 shows a case in which fracture begins at the right-hand skin before the higher quality of the steel on the left hand skin began to rupture, but the final fracture line was in a direction not at right angles to the line of stress but at the angle indicated in circle 3. It may be remarked that evenness or unevenness in texture as indicated by the position and angle of the final line of fracture has been found to be persistent along a test bar for several inches. The lower diagram of Fig. 9 indicates how the steel may be so uneven in mechanical quality as to break, say, $\frac{1}{4}$ in. above the plane of maximum stress. This, of course, means that, had the plane of fracture been coincident with the plane of maximum stress, that is, level with the face of the die, the number of alternations endured would have been sensibly less, and this + error may be approximately corrected by setting out a diagram like the lower sketch in Fig. 9. M is the line of maximum stress, Z the zero of stress. Draw a line along the plane of rupture parallel with the plane of maximum stress, then at any convenient angle draw a line from the zero of stress to the plane of maximum stress, on the point of intersection erect a perpendicular reaching the plane of rupture. Let the line x T be proportional to the alterations endured, then the result which would have been registered, had the fracture taken place on the plane of maximum stress, will equal x T$-x\,y$, y being the point of intersection of the angular line with the plane of rupture. The author here hastens to admit that theoretically the Wöhler test is perfect, whilst that of the author is theoretically hopelessly wrong, since it reproduces no known condition in engineering practice, nevertheless in many cases this test predicts the liability of material to fail in practice when the Wöhler test would confidently indicate that the metal was reliable. This apparently improbable enunciation, the author will presently prove experimentally by results obtained on unknown metals, over the preparation of which the author had no control, and distinguished from each other at the time the tests were made only by certain letters and numbers. As comparative bending tests made by the method of Wöhler and that of the author accumulated,

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it became evident that the former test was merely a reflection of the elastic limit; thus certainly indicating danger of fracture under excessive stresses, but insidiously concealing danger from other causes, which determine fractures generally attributed to fatigue. In fact, no matter how dangerously brittle the steel may be from chemical or physical causes, if such causes have co-produced a high elastic limit, the Wöhler test indicates the steel to be quite safe if stressed well short of that limit, when, as a matter of practical fact, it is inevitable that the steel must suddenly rupture sooner or later under stresses theoretically quite safe. For, perhaps, the strongest proof of the soundness of the foregoing statement, the author is indebted to his friend, Mr. J. E. Stead, F.R.S., who determined to test the points at issue on steels concerning whose highly dangerous character there could be no possible doubt. Mr. Stead accordingly made and tested at Middlesborough a series of mild steels with ascending phosphorus up to about 0·5 per cent. Of course, to deliver such a steel as that last named for use in the construction of engine parts, could be the act only of a metallurgical madman. Mr. Stead's steels were forged from 6 in. to 2 in. square, and were practically identical in composition, except for their phosphorus contents. Their general analysis registered :--

Carbon	0·30 per cent.	Silicon	0·21 per cent.
Mn	0·40 per cent.	Sulphur	0·058 per cent.

In No. 1 steel the phosphorus was 0·041 per cent.; in No. 2, 0·303 per cent., and in No. 3 0·509 per cent.

The static tests are embodied in Table VIII.

TABLE VIII.

Steel No.	Phosphorus per cent.	Yield Point. Tons per square inch.	Maximum Stress. Tons per square inch.	Elongation per cent. on 6 in.	Reduction of Area per cent.
1	·041	20·4	33·1	23·0	52·0
2	·302	25·4	39·9	23·0	45·3
3	·509	32·0	44·0	20·0	45·3

The Wöhler tests made by Mr. Stead give the results set forth in Table IX., the stresses being + and − 15 tons per square inch, *i.e.*, a range of 30 tons.

TABLE IX.

Steel No.	Phosphorus per cent.	Reversals of stress endured.	Ratio of Resistance to alternating stress.
1	·041	61,000	1·0
2	·302	167,000	2·7
3	·509	651,000	10·6

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The author's tests made by him at Sheffield University in complete ignorance of the nature of the steels registered the figures embodied in Table X.

TABLE X.

Steel No.	Phosphorus per cent.	Alternations endured in Test No. (under standard conditions)		Mean.	Ratio of Resistance to alternating stress.
		1.	2.		
1	·041	258	284	272	100
2	·302	188	212	200	73
3	·509	72	128	100	37

Speaking in round numbers, the Wöhler test indicated that a mild steel containing 0·5 per cent. phosphorus was, with equal stresses, ten times as capable of resisting alternating stress as a steel containing 0·04 per cent. phosphorus. The author's test, on the other hand, indicated that a steel containing 0·5 per cent. phosphorus had about one-third the endurance of a steel containing only 0·04 phosphorus. The curves of the yield points of the two alternating tests of the three steels are plotted in Fig. 10, (Plate XXIX.), where it will be seen that the Wöhler curve is similar in type to that registered by the yield point or apparent elastic limit, the author's curve being in an opposite direction and indicating what is well known to be the mechanical effect of phosphorus on steel.

The Wöhler Test applied to Copper Aluminium Alloys.

In the eighth report made to the Alloys Research Committee of the Institution of Mechanical Engineers by Dr. H. C. H. Carpenter and Mr. C. A. Edwards, from the National Physical Laboratory, it was observed by these authors in connection with rolled bars that the added aluminium remained dissolved in the cold alloy till the copper became saturated, namely, when about 7·5 per cent. of aluminium was present. The micro-structures of these alloys, up to and including the saturated alloy, all consisted of a single constituent presenting the ordinary allotrimorphic crystals of metallography, but on the addition of aluminium in excess of 7·5 per cent. a new dark-etching constituent, presumably rich in aluminium, appeared, and more or less perfectly enveloped the crystals of the saturated solution in a relatively brittle cement. A set of these alloys, in which the copper contained from 0·10 to 10 per cent. aluminium, was statically and kinetically tested by Dr. Stanton at the National Physical Laboratory. Dr. Stanton's machine embodies his ingenious development of that type of Wöhler test, in which the stress is evenly distributed over the whole area of the test bar. Dr. Stanton found that the alternating results were substantially a reflection of the apparent elastic limit in simple tension, namely, the endurance rose with the per-

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centage of aluminium to about 4·5 per cent., from 4·5 to about 6·5 per cent. it remained fairly steady, whilst from 6·5 to 10 per cent. the rising line of endurance became steep, reaching a maximum with the alloy containing about 90 per cent. copper and 10 per cent. aluminium. Thus, whilst somewhat faithfully reflecting the elastic limit (see curves plotted in Fig. 11, Plate XXIX.), the test had entirely failed to correlate itself with a remarkable change in micostructure when the aluminium in the alloy rose from 7·5 per cent. to about 10 per cent. It had been an instruction by the Alloys Research Committee that the alloys be also tested by the author's alternating stress method in use in Sheffield University. The tests were carried out by the author in absolute ignorance of the chemical, mechanical, and micrographical features of the alloys, and the results obtained, under the standard conditions already specified, are embodied in Table XI.

TABLE XI.

N.P.L. Mark.	Al per cent.	Alternations endured in Test No.		Mean Alternations endured.
		1.	2.	
29 R	0·10	308	266	287
25 R	2·99	330	334	332
23 R	5·07	658	606	632
20 R	7·35	1,530	1,260	1,395 *
18 R	9·90	707	607	657

* This alloy registered the highest resistance to alternating stresses ever observed by the author.

The mean results are plotted in Fig. 12 (Plate XXIX.), in which the upper micrograph shows the structure of the 7·38 per cent. alloy and the lower micrographic features of the 9·9 per cent. alloy. The author's test has sharply correlated its results with the striking structural changes in the alloys, and also shows by means of a sweetly flowing curve how the resistance to alternating stresses rises with the amount of aluminium dissolved in the copper, and reaches a maximum co-incident with the saturated solution.

Does High Elastic Limit necessarily make for Safety?

The author unhesitatingly answers the above question in the negative. It is, of course, certain that if a metal is subjected to a working stress not far removed from its elastic limit, such metal must inevitably fracture as the result of a complex final cause, of which the Wöhler phenomenon is the most important factor; but it is a great mistake to suppose that, because a steel of high elastic limit (associated

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with a satisfactory statical test) gives a good result under the Wöhler test, it will be, therefore, faithful in practice. It has already been shown that the obviously dangerous steels in which Mr. Stead raised the elastic limit by chemical means, namely, by the addition of highly excessive amounts of phosphorus, quite failed to indicate under the Wöhler test their inherent brittleness, which, had they gone into use, must soon have brought about their sudden fracture under alternating stress or shock, nevertheless some engineers have become so obsessed with the high elastic limit theory as to have altogether overdone it, and so stepped out of the frying-pan into the fire; for instance, with reference to holding-down bolts a pernicious practice of raising the elastic limit by mechanical strain has been practised to a considerable extent. Take as an example small bolts of $\frac{3}{4}$ in. diameter which have been hot-rolled in the ordinary way almost to size, and then cold-drawn down, say, $\frac{1}{32}$ of an inch, to their exact diameter. The final product is a clean smooth bar of steel of high elastic limit, capable of registering an excellent Wöhler test. This steel also makes excessively dangerous bolts. The failure of such bolts in practice has been frequently investigated by the author. For bolt steel he has specified a resistance to alternation under the standard conditions of at least 300 reversals before fracture. He recently tested some rolled and drawn bolt steel which had broken in use. Its most noticeable chemical feature was an excessive percentage of phosphorus. In alternation it behaved as shown in Table XIII.

TABLE XIII.

No.	Alternations endured under Standard Tests.	Ratio of Reversals below the specification.
1	76	75 per cent.
2	46	85 per cent.
3	46	85 per cent.
4	50	83 per cent.

Some time ago, to test this question, the author made the following experiments on acid steel $\frac{3}{4}$ in. round bolts, both as rolled and in the rolled and drawn condition.

The chemical composition of the four steels employed was practically constant, except in carbon. The silicon averaged 0·12 per cent., manganese 0·85 per cent., the phosphorus 0·044 per cent., and the sulphur 0·041 per cent. The carbon contents of the "A" steel were 0·26 per cent., "B" steel 0·31 per cent., "C" steel 0·41 per cent., and the "D" steel 0·49 per cent.

The tests are set forth in Table XIV.

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TABLE XIV.

HOT ROLLED SET.

Mark.	C. per cent.	Yield Point. Tons per square inch.	Maximum Stress. Tons per square inch.	Elongation per cent. on 2 in.	Reduction of area per cent.
A	0·26	24·1	35·3	34·5	61·6
B	0·31	25·7	37·6	31·8	59·6
C	0·41	28·9	42·7	26·5	48·0
D	0·49	29·9	46·9	25·5	50·8
		ROLLED AND DRAWN SET.			
A	0·26	36·7	38·9	19·0	50·8
B	0·31	35·1	41·9	17·5	47·2
C	0·41	38·9	46·6	16·0	40·4
D	0·49	38·0	52·4	15·0	42·0

The yield points had thus been raised by the drawing operation to the following extent:—

A 12·6 tons per square inch.
B 9·4 „ „ „
C 10·0 „ „ „
D 8·1 „ „ „

Under the author's standard alternating tests the results embodied in Table XV. were registered:—

TABLE XV.

Mark.	C per cent.	Yield Point. Tons per square inch.	Alterations endured.	Condition of Steel.
A	0·26	24·1	346	Hot rolled.
A	0·26	36·7	248	Rolled and drawn.
B	0·31	25·7	344	Hot rolled.
B	0·31	35·7	222	Rolled and drawn.
C	0·41	28·9	336	Hot rolled.
C	0·41	38·9	206	Rolled and drawn.
D	0·49	29·9	320	Hot rolled.
D	0·49	38·0	156	Rolled and drawn.

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Thus the mean endurance of the hot-rolled steels was 337 alternations of stress, that of the rolled and drawn steel was 208 reversals, a mean difference of 129 alternations. The rolled and drawn steels were 30 per cent. below the specified minimum endurance, whilst the hot-rolled steels were 12 per cent. above the specification. It should also be remembered that the test bars were turned to $\frac{3}{8}$ in. diameter, thus completely removing the skin of the bolts on which the mechanical injury caused by drawing would be at its maximum. The tests are plotted in Fig. 13 (Plate XXIX.), in which it will be seen that the erratic effect of the cold drawing has disturbed anything like regular co-ordination between the percentage of carbon present and the alternations endured. In drawing to a close his reference to the Wöhler test and its indications, the author cannot do better than quote a remarkable paragraph from an able paper on "The Production and Thermal Treatment of Steel in large Masses," by Mr. Cosmo Johns, of Messrs. Vickers, Son & Maxim, Ltd., read before the May meeting of the Iron and Steel Institute, 1904 (Journal of the I.S.I., No. 1, page 72, 1904) :—

"It may have been noticed that endurance tests have not been included in the tabular results given above. The reason is that in no instance have they shown any want of agreement with the static tests, while the mere recital of the number of revolutions a particular test-piece ran before it broke, would convey no information unless they could be compared with the results obtained on other machines under similar conditions, with the same ratio of load to elastic limit. The endurance is proportional to, and depends upon, the elastic limit."

Finally, if steels were liable to fracture only under the fatigue phenomenon of Wöhler, it is hardly an unreasonable hypothesis to put forward that under the author's tests, steels should, within structural carbon limits, register an approximately equal endurance, since in this method all steels are stressed to about their yield point, *i.e.*, a low carbon steel is stressed, say, to 15 tons per square inch, whilst a high-carbon steel would be stressed to perhaps 25 tons per square inch.

The Molecular Architecture of Mild Steel.

In a report to Mr. Milton, of Lloyd's Register, the author included the following paragraph :—

"The facts enumerated bring metallurgists face to face with a hitherto unsuspected phase of the crystallisation of iron ; namely that in over-heated mild steels, which necessarily leave the rolls at a high temperature, cool relatively slowly, and hence, slowly and more or less perfectly crystallise, after the microscopically visible crystallisation is completed, there takes place from a series of centres a secondary and microscopically invisible phase of crystallisation, which may be best described as a sharp, molecular development of potential cleavage planes. Steel in the centres of such developments will be brittle. The steel near their circumferences in which the molecular re-arrangement is only partially developed will be indifferent in quality,

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whilst steel beyond the radius of these developments of molecular cleavage will be good."

In Fig. 14 (Plate XXVIII.) the hypothesis above enunciated is shown in graphic form. The theory is at any rate not antagonistic to the facts mechanically observed in boiler plates, namely, that a good area will, under the author's standard test, endure in alternation about 340 reversals, an indifferent area, say, 180 reversals, whilst a very bad area may endure only 60 reversals before complete fracture. Since enunciating the cleavage plane theory the author has been fortunately able to prove experimentally that a crystal of wrought iron can develop absolutely perfect mineral cleavage, parallel to the faces of the cube. Mr. Vaughan Pendred sent to him a wrought iron bolt, which, after being in use for 30 years under practically no stress, fractured with a few taps from a hand hammer. On microscopically examining the fractured face, a crystal was discovered showing perfect mineral "step" cleavage. This crystal is represented in Fig. 15 (Plate XXVIII.). A piece of the broken bolt tore badly on the tension side when bent, but bent through an angle of 180° without completely fracturing. Tested in alternation under standard conditions with a piece of Farnley bolt iron for comparison, the last-named material broke after enduring 228 alternations of stress, whilst Mr. Pendred's bolt endured only 12 reversals, the lowest result ever registered on the author's machine in connection with structural materials.

Influence of the so-called "Fibre" on the Strength of Structural Steel.

The "fibre" of wrought iron is cinder. The fibre of steel is sulphide of manganese (MnS). Hence the inventor who can produce fibreless steel will deserve well of his country. Sulphide of manganese in steel was discovered in the laboratories of what is now the University of Sheffield about 1896, and on several occasions has been subsequently fully described by the author. Nevertheless, the exact effect of this constituent on the mechanical properties of steel is still imperfectly understood. Its deleterious effect in connection with ghost lines is broadly realised, but its influence on the behaviour of steel with varying directions of stress is little known. It may be well to remind engineers that in steel 0·05 per cent. of sulphur by weight is roughly equivalent to 0·25 per cent. of sulphide of manganese by volume, a quantity by no means insignificant. This constituent permeates considerable masses of structural steel in the form of millions of little pointed rods parallel with the direction of the forging. Thus in many cases these rods (which are equivalent to fissures filled up with a fused and brittle material) are stressed at about right angles to their length, a direction in which their harmful mechanical influence is at a minimum, but there are cases in which the plane of maximum stress will be parallel to the sulphide rods or fibre, and in such cases their mechanical weakening action on the steel is at a maximum, nevertheless static tests do not indicate any very serious weakening when the plane of fracture is parallel to the fibres. The figures in Table XVI. are typical of results obtained on a mild steel containing 0·04 per cent. sulphur, and consequently 0·2 per cent. by volume of sulphide of manganese rods.

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TABLE XVI.

Direction of Test.	Maximum Stress. Tons per square in.	Elongation per cent. on 2 in.	Reduction of Area per cent.
Longitudinal	28·0	33·0	55·2
Transverse or cross ...	27·2	28·0	47·7

The author, however, has found in three carefully carried out series of tests on mild steel shafts varying from 7½ to 24 in. in diameter that under rapid alternating stresses applied parallel to the direction of the sulphide rods, there is a startling difference in the capacity of the steel to resist such stresses, at any rate when accompanied by a small plastic strain.

The mean results obtained are condensed into Table XVII.

TABLE XVII.

Series No.	Alternations endured under Standard Conditions.		Remarks.
	Direction of Plane of Maximum Stress.		
	At Right Angles to MnS Rods.	Parallel with MnS Rods.	
1	283	193	Mean of 12 tests in each direction
2	293	118	„ 6 „ „ „
3	303	117	„ 6 „ „ „

It may be remarked that all the above shafts fractured in use. The direction of the sulphide rods in relation to the plane of maximum stress on the author's machine will be clear on reference to the diagram shown in Fig. 16 (Plate XXIX.). The twenty-four fractures of the bars tested with the maximum stress plane parallel with the sulphide lines showed "fibre," which was quite absent from the twenty-four bars tested in the plane of maximum stress at right angle to the MnS rods.

On Possible Remedial Measures.

A little time ago the author submitted to Sir William White a few of the somewhat disconcerting facts described in this paper, and, whilst Sir William White fully agreed that it was desirable to publish them, he pointed out that a more valuable feature in such a paper would be to propose therein the steps which should be taken to prevent, as far as possible, the manufacture of steel forgings of doubtful quality, for marine and other engineering purposes. It is easy to formulate airy proposals, provided that the proposer belongs to the class by which an eminent but

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gloomy author has declared our island is chiefly populated. Such might take up the case with alacrity, whilst another abstract class of the community, although popularly supposed to be provided with wings, would strongly hesitate to step in; but, as the author is well aware that steel makers will welcome any well-considered proposals to improve the quality, especially of large masses of steel, and engineers earnestly wish to secure adequate elasticity in their materials of construction, he, with much diffidence, will venture on a few suggestive remarks in the direction indicated by Sir William White.

Remarks on Ingots.—The necessity of rejecting a sufficient weight of the upper part of a large ingot to ensure the absence in the steel used for forging of liquated regions, pipes, and pockets is too well known to need any remark, but even in the lower portions of such an ingot, presumably of good steel, it is practically impossible with a mean sulphur and phosphorus of 0·04 to 0·05 per cent. each to avoid some segregation areas, if the ingot be allowed to cool at atmospheric pressure, hence the production of some ghost lines is inevitable; but if, as is sometimes the case, inferior steels containing up to 0·10 per cent. each of sulphur and phosphorus are accepted, the mischief is greatly aggravated. In the author's opinion in all heavily stressed engine parts the maximum phosphorus and sulphur should never exceed 0·05 per cent. each. Even with low sulphur and phosphorus it is desirable to reduce segregation to a minimum, and, so far as the author's knowledge goes, the only reliable and practical way to bring about this consummation is to cool the ingot under fluid compression, as long carried out by the Whitworth process or by recent developments of the original idea devised by Harmet and by Messrs. Robinson and Rodger, of Sheffield. But nearly one-third from the top of the Whitworth ingot there is a large bright cavity or pocket full of mixed gases consisting chiefly ot hydrogen and nitrogen, and in the vicinity of this pocket there is serious segregation, which is, of course, got rid of by cutting away the upper part of the ingot well below the pocket. The Whitworth ingot is exceedingly clean, this fact conducing to sound forging, and it is relatively free from segregation compared with a similar ingot allowed to cool spontaneously at atmospheric pressure. The maximum size of ingot to which the application of Whitworth fluid compression is effective is an open question.

The Harmet process of bottom pressure seems very successful in preventing segregation. The waste is much less than with the Whitworth process, although the skin is hardly so clean as that of the Whitworth ingot. Unfortunately the view expressed to the author by an experienced and competent expert is that a relatively small, say, a 30 ton ingot, must be regarded as the limit to which bottom fluid compression by the Harmet method can be effectively applied.

The Heat Treatment of Forgings.—In the author's view anything like a protracted cooling, especially at a low red heat, is dangerous, and likely to unduly lower the elastic limit of the steel under treatment.

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Three other methods* suggest themselves:—

1. When the forging operation is over, let the forging cool in air as far as possible out of draughts, and apply no further heat treatment.

2. Re-heat the forging to about 900° C., quench in oil, and finally "let down" by re-heating to a temperature between 300° and 400° C.

3. Re-crystallise the steel and remove stresses from the forging by heating to about 900° C., or at any rate above Osmond's top critical change point Ac_3, and then cool as quickly as the circumstances of the case will permit, in air; in other words, normalise the material.

In the opinion of the author method 3 is likely to give the best average results.

Mechanical Testing of Forgings.—The author regards kinetic tests as useful for experimental investigations, or occasionally desirable as an auxiliary method in cases of doubtful material. The static test must always be the main method of testing as between engineer and metallurgist, but the author most strongly advocates the abandonment of maximum stress as a base for the calculations of factors of safety. It has become urgent for the marine engineer to secure invariably an approximate measurement of the real elastic limit of the steel he uses. In connection with certain parts of machinery it is much easier to tender this advice than to show how it is to be followed under manufacturing conditions. It is, however, quite clear that all test pieces should be taken from the heat-treated forging before machining the latter, and that the portion taken for testing should be really representative of the mass of metal from which it is detached. As a base for calculations the yield point or apparent elastic limit offers the most promising features, since it is generally connected with a decisive drop in the beam of the testing machine, and is consequently almost as easily, though not quite so accurately, determinable as the maximum stress. The latter, of course, should always be noted so as to obtain a reasonable ratio between the yield point and the maximum stress, say, from 45 per cent. to 55 per cent. It would, however, be quite futile to attempt to lay down dogmatic regulations for this matter, because, steadily bearing in mind the obviously desirable end that the engineer should always have a reasonably accurate idea of the elasticity and, in special cases, of the resistance to alternating stress of the materials he fashions into such awe-inspiring forms, the points at issue must be settled on their merits by mutual and amicable arrangements between engineer and steel maker.

The author has to thank his colleague, Mr. J. H. Wreaks, for preparing the whole of the illustrations, and his Laboratory Engineer, Mr. J. Harrison, for assistance in perfecting the method of kinetic testing described in this paper.

* The author has purposely refrained from discussing the possibility of always securing chemically an adequate elasticity by the introduction of special elements. This aspect of the subject is a very large one, but is well worthy of serious consideration and investigation.

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DISCUSSION.

Professor J. A. EWING, C.B., LL.D., F.R.S. (Associate): My Lord and Gentlemen, I came here without any intention of speaking, but you have done me the honour to call upon me, and I am glad to have an opportunity of thanking Professor Arnold for having brought before us a number of points which are of fundamental importance in design so far as design depends upon strength. I am in full agreement with much, at least, of what Professor Arnold has said to-night. First of all, I agree with him as to the very unsatisfactory character of existing practice in regard to specifications of strength. I agree with him that it would be a good thing if we could get engineers to drop altogether the habit of taking the ultimate strength of a material as the main criterion of its value in respect of strength for purposes of design. The elastic limit is a far more important criterion, as Professor Arnold has insisted. But, of course, we have to recognise that in addition to the elastic limit we have this further immensely important criterion, the amount of plastic deformation that the piece is capable of undergoing beyond the elastic limit before it comes to complete rupture. If we could get specifications to recognise those two points, then I think we should be embarking on a practice very much superior to that which holds at present. What we should specify in a tensile test are (1) the elastic limit and (2) the total elongation. Those two things are much more valuable as a criterion of the merit of the material than the two which are commonly specified—namely, the ultimate strength and the elongation. I think that the whole subject receives much elucidation if one considers what it is that happens when the elastic limit is reached in the straining of a material. We now know that there is a sudden slip in a plane, or, rather, in a number of parallel planes, in each individual crystal; at all events, such slips then occur in some of the crystals of which the piece is composed. Up to that point in the straining of the piece, there has been a strictly elastic reversible action; there has been no set. When that point is reached, there is some minute amount of set occurring in each of the crystals within which these slips take place. From this point of view the elastic limit is really definite. It is definite as regards any individual crystal, although it may come at somewhat different loads in different crystals. Strictly, the elastic limit of the piece regarded as a whole would be the load at which the first of these slips occurs in any of the crystals which make up the piece. Probably that is lower than anything we are in the habit of recognising in an ordinary test as the elastic limit. What we usually call the elastic limit is a stage where slips begin in many of the crystals almost or quite simultaneously.

Now let us go further and inquire what it is that really causes fatigue. It appears from the researches which I carried out some years ago at Cambridge in conjunction with one of my students, Mr. Humfrey, that the condition which determines whether fatigue is to take place or not is simply this: has the elastic limit been passed in any of the crystals which are under stress? If it has, fatigue has been initiated, and then it is merely a question of how many repetitions of the alternating stress can be borne before the piece ultimately gives way. The slips which can be detected under the microscope are seen gradually to develop into positive cracks as the stress is repeated hundreds, or thousands, or hundreds of thousands of times, and finally the piece breaks when these cracks become sufficiently pronounced. I agree with what Professor Arnold has said about the pernicious practice of giving a piece overstrain in order to raise its elastic limit. It is true that by giving a piece overstrain, as, for instance, by stretching a piece of soft iron or by cold rolling, you may heighten the elastic limit in regard to one particular mode of static stress; but, nevertheless, you have deprived the piece of a portion of its reserve, and made it less fit for any useful purpose, except, possibly—and this is a reservation in which I think Professor Arnold will concur—in cases where it has to bear an absolutely static load. Even then, I am not certain that the piece is any better; and certainly, it is not better, but worse, if we have to deal with any variations of stress at all.

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The bad effects of over-annealing, in greatly lowering the elastic limit of which Professor Arnold has spoken with so much force, are, I think, quite intelligible when we consider the nature of non-elastic strain. Non-elastic strain occurs whenever we get slip beginning in any of the crystals. What is it that prevents slip in any crystal? There are two factors that make up the resistance to slip: in the first place, there is the inherent quality of the crystal itself. If you could take an absolutely isolated crystal—let us say, a crystal of ferrite, separated from all its neighbours—and cause slip to occur in it by straining in the crystal, you will find a resistance to slip which is due to the purely inherent quality of the stuff itself. But put that crystal into the environment which it has in an actual piece of metal, you then find that that same crystal is receiving support from all its neighbours, which has very greatly increased its resistance to slip. So we have those two factors constantly operating in every crystal of the piece—the inherent resistance, on the one hand, of the individual stuff of which the particular crystal is composed, and the additional support which it gets from all its neighbours, whatever they may be, whether crystals of ferrite or pearlite or anything else. Now, compare in this respect an over-annealed piece with a normal piece of steel. The broad characteristic of the over-annealed piece is that you have large masses of ferrite, often in single crystals and not broken up by a proper intermixture with pearlite, and consequently you find the second factor to be much less operative than it is in normal metal. In other words, the resistance to slip within the crystal then depends to a much larger extent upon the inherent quality of the crystal itself, and to a much less extent upon the assistance which it receives from its neighbours. The bigger the crystal is, the less the effect exercised by the neighbours in assisting it not to slip. I think we have in that, broadly speaking, the explanation of the much higher elastic limit which is found in normal steel than in over-annealed steel, and the still higher limit that is found when you undertake deliberate heat treatment with a view to bringing the steel into what is commonly called a "sorbitic" state. You then get a fine-grained structure in which there is an intimate mixture of pearlite with ferrite, which is the best condition for high elastic limit, and consequently to resist "fatigue."

I must not take up the time of the meeting further in discussing the points that Professor Arnold has raised, but I should like to emphasise this, that tests ought, in my opinion, to be directed far more than they are to a determination of what is the actual elastic limit of the piece. I am not satisfied to accept the yield point as the thing to be observed in testing, because in some materials there is no very pronounced yield point, and the less pronounced the yield point is, the greater is the chance of going a long way beyond the elastic limit before you recognise that you have passed it. If you are watching for a beam drop in a test, you may go a very long way past the true elastic limit before you see anything which you can call a beam drop, and in some materials there is no beam drop at all. Therefore, I think it would be well to introduce in the process of testing some very simple form of extensometer which would allow the elastic limit to be readily seen. In fact, this point was engaging my attention some time ago, and I then designed a simple extensometer for the purpose—namely, to allow the elastic limit to be readily noted in ordinary testing.

Mr. R. A. Hadfield (Associate): My Lord and Gentlemen, I can only re-echo Professor Ewing's remarks regarding the admirable nature of this paper. Coming as I do from Sheffield, I am, perhaps, rather partial to the work of Professor Arnold, who has done so much for us at our University; but, if all of you in this room knew how much he has done to assist our steelmakers there, you would understand my point of view. I can only add that this paper is another proof of the great value of the University and its metallurgical training. I heartily agree with Professor Ewing as to the use of the term "breaking stress." Certainly, that term as now used is often almost a misnomer. If one watches a tensile piece being broken, it is clear that the breaking stress is not correctly known—that is, as regards the actual stress upon the area of the piece at the time it fractures. As regards the elastic limit, I dealt with this point in the Forrest Lecture that I had the pleasure of giving to the Institution of Civil Engineers two years ago. I described a specimen of mild steel containing ·12 per cent. of carbon and ·28 of manganese, the other elements being of normal low percentage. From that specimen we

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obtained an elastic limit of 16 tons, with a so-called breaking load of 28 tons per square inch, elongation 35 per cent., and reduction in area 65 per cent. Under the notch test this specimen bent double cold. I heated portions of the same steel to a high temperature, about 1,200° Centigrade, and allowed them to cool very slowly. The result was that we got a material having characteristics such as those described by Professor Arnold; there was a drop of something like 7 or 8 tons in elastic limit, and a tenacity of 22 tons per square inch. There was a rise of no less than 10 per cent. in the elongation—that is, to the very high figure of 46 per cent.—and the reduction of area still remained very high—namely, 64 per cent. One would imagine that such a material was, therefore, owing to its high elongation, in a very much better condition to resist shock test. I do not know whether many of those present here will credit the fact that under the nick test it snapped like a piece of cast iron and did not bend more than a few degrees. This shows us, I will not say the misleading character of an ordinary tensile test, but that the ordinary slow pulling of a piece of steel, separating it into two portions, is not really an accurate guide, or, at any rate, not a sufficiently scientific guide for indicating the qualities of material for parts of machinery which are called upon to bear the very high stresses they have to do nowadays. I see some of our Admiralty friends present, and I should like to ask their special attention to the fact that they are calling upon steelmakers every year for steel having higher qualities of resistance; but my own feeling is that it is almost impossible to get this without very considerably raising the quality of the steel. Now, what do I mean by raising the quality of the steel? Unfortunately, I mean it in a very practical pounds, shillings, and pence point of view—I mean raising the cost of the steel. I believe steelmakers to-day can give the Admiralty or other Government Departments in this great Empire of ours any steel that they like to call for, but they cannot do it except at a largely increased cost. It takes an expenditure of more than money to produce material of the highest quality, and therefore we must ask those in authority, who call upon the metallurgists to give better material, to also allow some margin beyond the cost of production, in order that we may be able to carry out certain processes which involve increased cost. If this increased cost is paid, I believe steel can be made which will stand any desired test, and we should not then hear of so many breakdowns. I say this after spending a large amount of time and money in experiments of various kinds, and I think my friend Professor Arnold will agree with me.

Reference is made on page 265 to the fact that mild steel is supplied with a composition and varying percentage of manganese from ·25 to ·8 per cent. Professor Arnold says, very rightly, that he prefers the higher percentage. In that fact alone we have, I think, some explanation of the large number of variations, or so-called variations, in the nature of steel. One maker uses ·3 or ·4 per cent. of manganese, another uses ·8 per cent., and, as manganese has a great influence upon the physical qualities, one cannot expect to get the same tests from each class of steel. Perhaps to those who do not make steel, ·4 per cent. difference of manganese does not seem much, but it has this important property, that you cannot, with the same kind of annealing, normalise a steel with ·4 per cent. of manganese as you can with ·8 per cent. of manganese. Therefore, when different steel works employ steel containing different percentages of managnese, the same kind of heat treatment in each case will not give the same results, and the same specification will not meet the conditions prevailing at each works. I see in a footnote to page 282 that Professor Arnold refers to the fact that he has not touched upon the question of special steel, and I quite understand his reason for not doing so. But perhaps I am a little freer to speak on that point than he is, and I would ask you a very important question. We all hear a great deal about motoring nowadays, and, with all due respect to our English makers, who are coming on rapidly, I think the best motor is still produced in France. Now, what has brought the French motor engine to the high state of perfection it now occupies? I say with full conviction that, as far as I have means of judging, it is largely owing to the attention which the French steelmakers have paid to the production of special steel. Great credit is due to the French scientists, such as Professor H. Le Chatelier, Monsieur Guillet, and others, who have worked hand in hand with the manufacturers, and enabled them to produce those special steels. A German friend of mine, who had been paying a visit to one of our leading automobile makers, told me he still found that sufficient attention was not being paid to this question of the use of special steels, including steels having a high elastic limit. It is necessary that the steel should not only have a high elastic limit,

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but, at the same time, great toughness, and it is quite possible, by the use of special steels and by paying proper attention to heat treatment, to obtain these qualities.

This leads me to a subject which hardly concerns the present paper except indirectly. Most of us know of the excellent work that is being done in Sheffield University as regards scientific metallurgical training, over which my friend Professor Arnold presides. We also know that in London it is proposed to enlarge very considerably the scope and field of operations in the Imperial College of Science at South Kensington. But the Imperial College of Science has not the necessary means, nor, if I may put it in a technical way, the necessary plant, for carrying on metallurgical training as regards iron and steel. Why should not the Imperial College of Science and Applied Science Department of Sheffield University work hand in hand? These two great establishments in London and Sheffield, by working hand in hand, could bring about a great advance in metallurgical training in this country, and we in Sheffield are quite prepared to do our part. We are delighted to know that Dr. Bovey has come over from the McGill University to take charge of the Imperial College of Science, and I only hope that it may be possible to bring about satisfactory arrangements between these two bodies in London and Sheffield.

Mr. C. E. Stromeyer (Member of Council): My Lord and Gentlemen, the thanks of the meeting are certainly due to Professor Arnold for bringing forward this question, but I am not sure whether engineers will quite agree with his recommendations either as regards the importance to be attached to the elastic limit, or to his objection to overstraining the material before it is put into use, because everybody knows that chains, boilers, and gun-barrels, and a good many other structures, have all to go through a proof stress before they are put into use, and that proof stress artificially raises the limit of elasticity very considerably. On page 276, Professor Arnold refers to some bolts that had been cold-rolled, but it appears that those which suffered most contained a high percentage of phosphorus, and I think that in this impurity, and not in the cold rolling, may be found the true cause of their bad behaviour. This is an important point, for I understand that the automatic screw-cutting machines, as now designed, are not quite strong enough for screwing English steels, and that special steels are being sold to be used in these screw-cutting machines. It seems to have been found by experience that the more phosphorus there is in the steel the more easily it can be cut, and the result is a natural tendency to use highly phosphoric steel for screw bolts, and these not unnaturally turn out to be brittle in use. I think there is a serious danger of steel manufacturers gradually being persuaded to supply steel containing more phosphorus than is desirable. Then, as regards the ultimate stress, I am afraid that engineers will not feel inclined to depart from the practice of using it as a standard, because it is a simple mechanical test for the amount of carbon in the steel. Engineers have, I think, in all ages, started from what happens in practice, and not from laboratory tests. For instance, if a shaft fails, as in the cases that Professor Arnold has mentioned, one would wish to experiment as fully as possible with that particular shaft, and devise tests which in the future would detect such bad steel. The test usually follows the practical experience, and not *vice versâ*. Our present tests were devised after mild steel had been made, and had proved itself reliable. I quite agree that one should not work up to the limit of elasticity, but it is a less definite criterion of the chemical constituents of the material than the ultimate stress. The usual quality tests are tenacity, elongation, and temperature bending, and these three results give one a very fair idea of the carbon, manganese, and remaining impurities. Unfortunately, they do not give one any indication of the percentage of phosphorus, which is the most harmful constituent; and if Professor Arnold's test will tell one the percentage of phosphorus, it will have done something that everybody has been anxious for. I certainly like his test very much, and it has this advantage over the Wöhler test, that by straining the material beyond the elastic limit, anything in the steel that is brittle, any crystals or nodules, which are, perhaps, of a highly phosphoric composition, would break up, whereas the Wöhler test merely determines a relation for all steels between the elastic limit and fatigue. That is why I hope that Professor Arnold will make further experiments, and, specially, that he will compare them with the actual failures which have taken place; and also with cases where shafts and other structures have lasted for an abnormally long time. Occasionally there

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are cases of that sort, and I think they are almost as important as those where the material has failed quickly.

Professor J. O. ARNOLD: My Lord and Gentlemen, I am extremely gratified by the very interesting and valuable discussion which has taken place, and I am more than pleased to find that Professor Ewing is so much in accord with my views. We have practically no points of difference, except, perhaps, in one respect, where I might suggest that the theory of slip, which no doubt has a very great deal in it, will not apply to a case like that figured on Fig. 15 (Plate XXVIII.), where we have an absolutely perfect mineral cleavage unpreceded by any slip whatever. I am in perfect agreement with Professor Ewing where he says it is an important matter tc know the amount of plastic deformation the material will stand before breaking; and we have a remarkable example of it in this paper on page 275 (which time precluded me from reading) in the copper-aluminium manufactured at the National Physical Laboratory, where an aluminium-bronze endured the highest number of alternations ever registered, viz., a mean of 1,395, being during that time deformed plastically. That is a remarkable case, and I fully agree with Professor Ewing, that is a new phase of the case which requires urgent investigation. With reference to Mr. Hadfield's remarks on the influence of manganese, I would fully confirm them, and point out that manganese has three functions. It raises the elastic limit of the ferrite and also of the pearlite, and, moreover, if in sufficient quantity, it annihilates that dread enemy of the steel maker, oxygen, from the material. With reference to Mr. Stromeyer's remarks, I would call his attention to the fact that the tables given (which are really condensed tables of the results given on page 277) show the results obtained on a steel low in phosphorus, which was rolled and drawn $\frac{3}{4}$ inch in diameter, which has been turned down to $\frac{5}{8}$ inch in diameter, and from which the whole of the plastically strained skin has consequently been removed; and yet, right through to the middle of that bar, we have a deterioration in quality, so that in the whole of the cold-drawn bars, the steel has fallen far below the limit of any specification which would pass only reliable steel.

The PRESIDENT: I am sure we are all very much obliged to Professor Arnold for his most interesting paper.

Mr. J. FOSTER KING (Member of Council) sent the following written contribution to the discussion: Because Professor Arnold's paper appears to me to be of great general interest, although much of the subject matter is, no doubt, special to the metallurgist, I must confess to a slight feeling of disappointment that the discussion has not drawn a larger proportion of expert opinion as to its direct bearing upon the practice of those who make and use steel, as well as those who certify as to its quality. It does not appear to me that the question as to whether factors of safety are to be based upon the elastic limit or the ultimate strength of steel can emerge from the academic stage until it is proved that, under ordinary conditions of manufacture and treatment, it is unsafe to assume that the average stress upon the material exceeds half the so-called breaking stress before there is any stress which matters. The essential point for the user of steel is not so much whether the elastic limit of the testing house is, or is not, the true elastic limit of the material, but whether its ratio to the breaking stress is, or is not, a sufficient guide to the safe sizes of material for bridge, ship, or forging. Even if it be true that laboratory testing shows that every stress may produce strain which is apparently permanent, it would be of great interest to hear whether, in Professor Arnold's opinion, and that of other experts, the success which has attended the enormous mass of steel construction now in being has not proved that, in service, steel has such power of recovery as to make the above assumption reasonable, and the apparent elastic limit a sufficiently safe guide to the just relationship between theory and practice.

With regard to the remarkable test results which Professor Arnold has obtained from steel which had been subjected to particular heat treatment, the point for the user is whether or not the ordinary methods of testing will show if such material is fit for its intended purpose. So far as the examples given in Tables III., V., and VI. are concerned, while the softened steel might not have been rejected on the tensile tests, certainly the ratio of the observed elastic limit to the minimum stress would have

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caused rejection, so that its more common use is thereby suggested. In addition to the treatment to which it was subjected, the material tested by Professor Arnold was probably somewhat abnormal in composition, and it may, therefore, be of some interest to follow some experiments on mild steel of ordinary composition which have been recently carried out. A plate of ship steel, which had probably

TABLE OF TESTS.

No.	Tons per square inch.	Elongation % on 8″	Observed Elastic Limit.	Contraction of Area %	Ratio of Elastic Limit to Ultimate.	Silicon.	Carbon.	Phosphorus.	Sulphur.	Manganese.
1	33·55	25·0	18·55	45·24	55 %	·043	·260	·062	·053	·620
2	31·75	26·75	17·90	47·56	56·0	·040	·275	·062	·043	·600
3	29·35	28·50	15·70	51·70	53·5	·040	·275	·062	·046	·630
4	32·36	27·0	17·16	45·73	53·0	·030	·200	·056	·056	·630

been put into stock because the tensile strength was above registry limits, was first tested as made (results No. 1 in Table), and then put in the annealing furnace, where the temperature was gradually raised to 750° Cent. The plate was soaked in this temperature for two hours, was then removed, and allowed to cool naturally in the open air. Tests were taken from the plate so treated (results No. 2 in Table), and it was again put in the annealing furnace, and the temperature was raised to 950° Cent. for a short time. The plate was then allowed to cool slowly in the furnace until it had been brought back to the temperature of 300° Cent. It was then withdrawn, after having been in the furnace for over fifteen hours, and allowed to cool naturally, and test pieces were again taken (results No. 3 in Table). A set of test pieces from the same plate were then put in the annealing furnace for the third time, and the pieces were left in the furnace for about two and a half days, *i.e.*, they were practically in the position of a forgotten plate exposed to the full heat of the furnace, which, being in use for other purposes, alternated between 950° and 500° Cent. The test pieces were allowed to cool naturally after removal, and the results of the tests are No. 4 in the Table. In each of the first three cases the different tests under each condition so closely resemble each other that the mean given in the Table may be accepted as having eliminated personal error. It will be observed that the effect of the first two processes of heat treatment has been to reduce the tensile strength and the observed elastic limit in each case. With regard to the fourth set of tests the results were not quite so regular, probably on account of the difficulty of removing all the scale caused by the heat treatment; but the tensile strength appears to have been raised very nearly to the original amount, and this is coincident with the disappearance of ·075 per cent. of the carbon in the material. The ratio of observed elastic limit to ultimate stress does not vary appreciably during the whole series of tests and the cold bends in each case were perfectly satisfactory, so that the results suggest that the ordinary steel of commerce will stand a very great deal of what may be called "heat abuse" before suffering to an extent which would justify rejection under ordinary tests.

The very interesting series of tests by Professor Arnold, which show the effect of introducing excess phosphorus, touches a matter which is possibly even more interesting than the possibility of non-discovery of maltreatment by heat through ordinary tests. While Table VIII. shows that the tensile tests might not have done so, the ordinary cold bend tests would probably have ensured the rejection of the particular material represented in this case; but it appears to me that steel makers can obtain such wonderful results by varying the chemical composition of steel, that the possibility of manufacturing steel which may comply with the ordinary physical tests, and which may

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yet be unreliable in use, is not an altogether remote contingency. If Professor Arnold, by his method of testing, under alternating stresses, can secure an effectual safeguard against such dangers as well as that of cold rolling, it is obviously a method which deserves earnest consideration. It would be of considerable value to learn from Professor Arnold what the adoption of his machine would mean in cost and facility, say, in comparison with ordinary bend-testing machines. Is the method one possible for commercial, as well as laboratory use? While I am not aware that the small percentage of all the steels submitted to the inspection of Classification Societies which turns out to be defective when being worked is sufficient to justify many doubts as to the efficacy of existing methods of testing, the existence of some failures is the strongest justification for the consideration and adoption of any economical and satisfactory method for preventing their recurrence.

Professor ARNOLD has sent the following written addition to his reply:—In reply to the valuable criticism of Mr. J. Foster King, I am in agreement with many of the views expressed by that gentleman, but would point out that Mr. King has overlooked the fact that the steels referred to in Table III. were experimental steels not subject to inspection. The steel in Table V. was a steel casting, concerning which it is most unusual to specify any elastic limit. The steels of which the mean results are given in Table VI. did as a matter of fact pass inspection, went into use, and there fractured. In conclusion, I might reply to Mr. King's question as to the commercial, as well as the laboratory use of the machine, that it is already in use at two of the largest steel works on the Continent.

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THE MODERN DEVELOPMENTS OF THE MARINER'S COMPASS.

By J. C. Dobbie, Esq., Associate.

[Read at the Spring Meetings of the Forty-ninth Session of the Institution of Naval Architects, April 10, 1908; the Right Hon. the Earl of Glasgow, G.C.M.G., LL.D., F.R.S., President, in the Chair.]

None of the many practical problems connected with magnetism are of greater interest or importance than those which are concerned with its application to the mariner's compass. The mariner's compass has been the subject of more speculative inventions than perhaps any other instrument, and it has occupied the minds of many inventors besides those directly connected with its construction. The problem of securing an efficient steady compass is one which is always more or less with us.

By a "steady" compass is meant one in which the card will not oscillate in a seaway, and so make steering difficult or impossible. The employment of iron, and especially steel, in the construction of modern ships, and the difficulties due to their increased speed, have materially complicated this problem.

The invention of the compass is ascribed to Flavio de Gioia, or Flavio of Amalphi, about 1302, but it is now well established that his improvement was the poising of the magnetic needle of the compass on a pivot. Gilbert, in *De Magnete*, says it was brought from China to Europe about 1260 by Marcus Paulus, a Venetian, commonly known as Marco Polo. This is probable, as early Europeans used the same device as the Chinese, viz., a magnet floating in a basin of water by means of a straw, or of a piece of cork or wood. The Chinese, however, used 24 points, and Flavio 8 points, as against the 32 points now employed.

It is a well-established fact that the directive force of a compass is necessarily lowered through the influence of the iron or steel of which ships are constructed. This influence is minimised by placing the compass in what is known as a "favourable" position in the ship.

The adjustment of the compass, *i.e.*, the compensation for the ship's disturbing force, resulting in the more or less complete restoration of the earth's magnetic force, has reached something like perfection on the lines laid down by the Astronomer Royal of his day, Sir George Airy—known as the father of the system of compass adjustment—and by Mr. Archibald Smith, of Jordanhill, Glasgow. Their views were embodied in practical form by Mr. John Gray, of Liverpool, and in later years by

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Sir Wm. Thomson (Lord Kelvin) and others. The use of compensating magnets for the compass was, however, first suggested by Barlow.*

The transitory deviation, due to what is styled sub-permanent † magnetism—a disturbance due to temporary magnetisation of the ship's hull, which develops after a change of course—was in the main got over, originally by the employment of the mast compass, and latterly by placing the standard compass on the top of the wheelhouse, or other structure, as far removed as possible from the magnetic influence of the ship's hull.

While these factors were successfully dealt with, and greater steadiness of the compass secured by thus increasing its directive force, the disturbance due to the vibration or rolling of the ship remained to be dealt with, and in recent years this has been practically the only serious problem left to solve, at least in merchant ships. The method devised to deal with this problem has been the adoption of some system of suspending the compass bowl in the binnacle, or the card in the compass, but the systems hitherto employed have not been entirely satisfactory, especially in modern fast ships, and so some additional method became necessary.

As the adjustment of the compass has so important a bearing on the whole subject, the steps taken to put Sir George Airy's theories into practical shape may now be described. Gray, in the sixties, was the first to employ a binnacle with the semi-circular and heeling magnets all enclosed in the binnacle casing, together with quadrantal deviation correctors composed of boxes filled with soft iron chain, or pieces of cast-iron shaped like clock weights, as recommended by the Liverpool Compass Committee; to the Secretary of which, Mr. W. Rundell, we owe so much. It was not until Lord Kelvin introduced his famous compass that correctors for quadrantal deviation took the improved form of spheres, and that the Flinders bar was, in its present form, attached to the binnacle casing. The Flinders bar is the invention of the great explorer Captain Flinders, and consists of a soft iron bar placed, in most cases, in front of the binnacle, to compensate for the component part of the semi-circular deviation caused by the vertical soft iron of the ship—constant *c* in the Admiralty Compass Manuals. The ship's vertical soft iron must necessarily change its magnetic force with change of geographical position, and the Flinders bar is placed vertically in such a position, relatively to the compass, as to counter-balance the changing value of this vertical soft iron. In its original form the Flinders bar was separate from the binnacle casing, and, although inconvenient, it is for some reasons the better method; but I shall refer to this later. Thus we have Thomson's system of compass and binnacle, the first in which were collected together all the elements necessary for complete compensation. His compass, as is well

* Barlow's suggestion is described in "Electricity and Magnetism," by Guillemin, edited by Professor Silvanus P. Thompson.

† Known variously as "Retentive Magnetism," "Gaussin error," or "Hysteresis," but none of those terms is quite descriptive of this error.

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known, is fitted with a light card with short needles, to allow of the more perfect adjustment for quadrantal deviation (Fig. 1, Plate XXX.). In his own language, his compass and binnacle were designed to put Sir George Airy's theories into the best practical shape. It is worthy of note, however, that short needles were used by Mr. W. Rundell for the steamer *Scotia's* compass as far back as 1865.* With the growing vibration of steamers the problem of producing a "steady" compass was however, always cropping up.

As already mentioned, the Chinese apparently used a crude form of liquid compass. In fact, liquid compasses in some shape or form have always been in use, and some thirty or forty years since, the Ritchie (of Boston) spirit compass, an American invention, held the field. Gray, of Liverpool, and other inventors, while employing a dry compass—*i.e.*, one in which the vibrations of the card are damped down only by the air in the bowl, floated one bowl inside another in liquid, and this device was more or less successful.† But by far the most successful suspension for a dry compass was the Thomson grummet ring (Fig. 2). This suspension is an elliptical ring composed of one length of brass or copper wire twisted several times round a core wire in such a manner as to give considerable elasticity. From this ring are hung athwartships, exactly opposite one another, chains with stirrups in which the trunnions or knife-edged journals of the gimbal ring rest. On the under side of the ring sockets are fixed, exactly fore and aft opposite one another, which rest on small balls fixed to the binnacle rim. Figs. 3 and 4 show other systems of spring suspensions, but it is a matter of doubt whether those are improvements on the grummet ring suspension. In recent years, however, even this ingenious arrangement has failed to meet the increasingly onerous conditions, and apparently the suspension of the card in liquid is the system to which we must look for the best practical results.

Whether the failure of the dry compass to cope with the new conditions arises from the fact that the construction of the Thomson card gives it something of a flywheel action, in turn partly braked by the paper rim, or from the fact that spring suspensions of any kind are not constant, it is certain that when the card is suspended in a suitable liquid, which will not change its condition, the suspension is a constant one. In the liquid compass the card is not actually floated by the liquid, but is sufficiently heavy to poise on the central pivot.

It is difficult to ascertain the exact effect of the new conditions which militate against the steadiness of the dry compass; but, so far as fast steamers are concerned, the opinion is that speed, combined with vibration, causes the cap which supports the

* With the advent of iron ships the single needle was discarded, and magnets in pairs 60° apart were attached to the compass card.

† The first suspension of compasses by means of gimbals to allow of the compass always taking up a horizontal position, known as "Cardan's Suspension," is of such importance that it is in use in all compasses. The suspensions mentioned in this paper are for another purpose, viz., intercepting the disturbance of the compass card caused by vibration, &c.

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card to lift slightly from the pivot and fall back again. This would quickly set up oscillation on the card, and accentuate any flywheel action.

I have known of compasses suspended by spring suspensions being actually thrown out of the gimbals. This was possibly due to the movements of the ship and the movements of the spring suspension coinciding.

An extraordinary incident came under my notice not long ago. In a London steamer, in which the standard compass was placed about 10 ft. from a steam whistle, the card was subject to violent oscillations when this whistle was blown. Another compass and card placed in exactly the same position showed no oscillation, and the theory put forward to account for this was that there was some relation between the sound vibrations of the whistle and the tremors of the compass bowl in the former case which did not exist in the latter. I am inclined to think, however, that what really happened was that vibrations were set up in the glass of the bowl which caused the pipe, projecting downwards from the centre of the glass to prevent the card rising, to strike against the cap, and so set the card in motion. I have not had an opportunity of trying this compass bowl with a different glass, but I imagine the disturbance arose from the glass and not from the bowl.

Captain E. W. Creak, C.B., R.N., in his "Elementary Compass Manual," warns us that "the mariner's compass is at the best a delicate instrument. A fibre that can hardly be seen by the naked eye, attached to the edge of the card and touching the side of the bowl, is quite enough to overcome the earth's directive force on the needles, and stop the card, whilst the ship's head has turned through 10° or more. The earth's magnetic force may then assert itself, and the card go off with a swing."

Considering all the circumstances enumerated, some of us have come to despair of ever getting a dry compass to work satisfactorily under every condition, and have been driven to experiment, more particularly, with the liquid form of compass.

The objections to the older forms of liquid compass were the interference with perfect adjustment due to the length of the magnets on the card, necessary to give it sufficient directive force; and the motion set up on the card by the annulus of liquid disturbed by the motions of the bowl, when a ship turned quickly in azimuth. In the new form of compass now to be described, these objections are overcome.

Previous to the end of last century, Captain Creak constructed a compass bowl, slightly hollowed out opposite the periphery of the card, leaving a greater distance than usual between the wall of the bowl and the periphery of the card, and undoubtedly this was a step in the right direction.

In 1901 the writer took out his first patent for a form of compass, the main feature of which is that the card is of such small diameter as compared with the bowl, that it is absolutely withdrawn from the annulus of liquid disturbed by the

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motions of the bowl. After being subjected to the most severe tests, this class of compass has been adopted by the British Admiralty for service use, and is to be fitted for the standard and steering compasses, in future, in the Navy.

The objections to this compass are the small size of the card, and the error of parallax, arising from the edge of the card being so far removed from the usual lubber line on the wall of the bowl. I provide for this by introducing a special lubber line, or pointer, in close proximity to the card, to represent the ship's head. In a later development I use a ring form of card, as shown in the illustration, marked with degrees and cardinal points only (Fig. 5, Plate XXX.). The degree divisions can be made so prominent that a steersman can readily see them, and these show up better in the absence of the darkening effect of the points and half-points. At first a magnifier was employed to read the card, but this was inconvenient, especially when using the azimuth instrument, and is rendered unnecessary by employing this style of card.

It is certainly an innovation to use a card marked with degrees only; but, as steering to degrees is now universally in use, the elimination of quarter-points and half-points is of little consequence and not injurious. As the degrees in all cards are nearest the periphery and quite close to the lubber line, this new arrangement should make for closer and more accurate steering than formerly.

Perhaps the best evidence of the superiority of this class of compass is afforded by the result of experiments made by Mr. Dugald Clerk, the writer, and others. It was found, with a compass constructed with a card of sufficiently small diameter as compared with that of the bowl, that the card was not deflected appreciably, even if the compass were turned through the whole 360° in a few seconds. As the fastest steamers afloat take about 50 seconds to complete a round turn, it is obvious that any disturbance from the turning movement of the ship is entirely eliminated.

A further advantage is, as with all liquid compasses, that magnets of great directive force are employed. With the Thomson and similar compasses where light cards are used, the magnets or needles are necessarily of small directive force, as Captain Creak points out in the paragraph from the "Elementary Compass Manual" already quoted. But his warnings do not apply to this compass in which powerful needles are used, and the periphery of the card is well withdrawn from the wall of the bowl. This is a comparatively robust instrument.

Returning to the subject of adjustment of the compass: the main reason for the adoption of short needles is that, if they are over a certain length, they magnetise by induction the quadrantal correctors. I do not think, however, it is so well known that the Flinders bar sometimes gets magnetised in a similar manner by the semi-circular correcting magnets. In my own experience I came across a case in which there was some obscure disturbance of a compass. It occurred to me to increase the distance between the lower end of the Flinders bar and the magnets, when I found that the disturbance disappeared. To obviate this danger I should like to see the Flinders bar

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kept separate from the binnacle casing in accordance with the original practice of Captain Flinders. Illustrated in Fig. 6 (Plate XXX.) is shown a modern binnacle, where A is one of the receptacles for magnets, B a quadrantal corrector, and C the casing for the Flinders bar.

It is a true saying that there is nothing new under the sun, and to-day we have the return of the liquid compass—not in the crude form employed by the Chinese, but constructed to suit modern conditions.

Naval architects will readily appreciate the reasons for the success of this class of liquid compass, as they have to deal with a similar problem in connection with the trial trips of steamers, which must take place in sufficiently deep water, to avoid the drag, due to friction or some similar cause, which lowers the speed. In the same way this compass is constructed so as to have deep water all round the card, and also above and below it, in order to eliminate "the drag."

I have designed an azimuth circle, as shown in the illustration (Fig. 7), which permits of bearings being taken at high speeds—which is not always possible with the form of azimuth most commonly in use—and also without removing the binnacle top, an important factor in bad weather. Captain Creak has a similar azimuth circle for taking instantaneous bearings, designed many years ago.

DISCUSSION.

Captain L. W. P. CHETWYND, R.N.: My Lord and Gentlemen, the author may be congratulated on the skilful manner in which this paper has been prepared. It would, I feel, be difficult from his point of view to make more of the subject than has been done. The matter may, however, be approached from other points of view, and, if such is the object of a discussion, I may be able to offer some remarks of general interest. With your permission, I will divide the subject under two heads—the technical and the historical, and I propose to offer a few remarks on the technical side first. Referring to the author's remarks as to placing the compass in a favourable position, this merely means that the compass should be at such a distance from any iron or steel fittings that the changes of magnetisation which, from one cause or another, take place in the iron, will not cause any appreciable change in the magnetic field at the compass; in other words, the further (within reasonable limits) the compass is from any iron or steel, the better. Closely connected with this point is the sub-permanent magnetism referred to in the paper, which is the principal cause of changes of deviation in a properly adjusted compass. There are no means of adjusting for this cause of error, but, if the compass is far enough away from the iron, the changes are unimportant. In all probability, however, we are within measurable distance of overcoming this source of error by the introduction of non-magnetisable steel, as considerable advance has been made in this direction in recent years. It is hardly correct to say that these changes of deviation develop after a change of course, though in one or two special cases their effect may not be noticed until after the ship's course has been altered. In no sense, however, can this cause of change

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be referred to as causing the compass card to be "unsteady" in the ordinary acceptance of the word. The causes are gradual, and so are the effects. Unsteadiness, or, to use a more descriptive term, oscillation, of the card is the result of mechanical causes and not (except in the particular case of bad adjustment) of magnetic causes. Oscillation is undoubtedly brought about by the shifting of the cap on the pivot, due to the vibration of the latter. The action appears to be the same as when a juggler spins a plate on the point of a stick, except that, on account of the directive force of the magnets and the small inertia of the card, the latter does not, as a rule, complete the revolutions, although it has on several occasions been known to do so. The magical effect of the steam whistle in causing an oscillation of the compass card was, I would suggest, possibly due to the compass bowl not being airtight, and therefore the vibrations set up by the whistle would find more or less free access to the compass card and cause it to oscillate. This is a defect which may not infrequently be found in compasses of inferior manufacture.

The necessity for using small needles was, I believe, not entirely due to the necessity for producing a light card, as suggested by the author of the paper. The small needles of low power were necessary to avoid inducing magnetism in the soft iron correctors used in adjustment. Such needles of low power necessitated the use of a light card, otherwise the inertia of the card would overcome the directive force of the needles. Thus it is the weakness of the needles which necessitated a light card, and not the lightness of the card which necessitated weak needles. There were, of course, other advantages gained by the use of the light card, but I need not go into them here. The serious objection to powerful needles is, as has been stated, that they induce magnetism in the soft iron correctors used in adjustment, and this is such a very important point that I should like to ask Mr. Dobbie what is the magnetic moment of the combination of powerful needles which he tells us are employed in his compass. As regards the magnetism induced in the Flinders bar by the adjusting magnets, I have no experience of semi-circular magnets, but, so far as the horizontal bar magnets usually employed are concerned, the induction of the Flinders bar is not of very serious importance, as the effect is merely to increase or diminish the effect of the bar, and this increase or decrease remains constant so long as the position of the horizontal magnets remains unchanged. In a few special cases, principally on board battleships and cruisers, where it is necessary to correct large heeling error by several vertical magnets, the position of which has to be changed on any change of latitude, the induction of the Flinders bar is of somewhat greater importance; but this cause of error is by no means obscure, and is more or less well known and allowed for.

The proposed compass card graduated in degrees only would, if adopted, certainly be an innovation. There are, however, many objections to it, which, I think, will prevent its adoption. Amongst these objections may be mentioned that the direction of the wind is always referred to by the points of the compass; also, in many cases, the magnetic bearings given on charts and in sailing directions are also given in points, so that to abolish the points on the compass card would not only entail large expense in reprinting documents, but would, I feel sure, not meet with the general support of those who use the compass.

I will now turn to the historical aspect of the paper. I have recently had occasion to investigate the history of the liquid compass, and some of the results of my inquiries may be of interest, as they will considerably amplify the information so interestingly put forward in this evening's paper. No history of the liquid compass can be complete without a reference to the earliest-known description of a compass. This well-authenticated record is contained, as Professor Sylvanus Thompson tells us, in a letter written in 1269 by Peter Peregrinus to Sygerus. In this letter there is not only a very clear description of the ancient compass consisting of a piece of lodestone fitted in a wooden jacket or float and floating in liquid, but there is also what is generally accepted as the earliest description of the pivoted compass. The needle is a piece of magnetised iron wire, and is inserted in an upright spindle; there is no card attached, but the upper edge of the bowl is graduated in degrees, in a manner similar to that of our present compass cards, and the observer is instructed to turn the bowl so that the meridian or zero points of the bowl are in line with the direction of the needle. Peregrinus, by the way, also mentions that it is an advantage to have the bowl of some transparent material. The adoption of a compass card came later, the earliest

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example known being the one ascribed to Jacobus Giraldi in the year 1426. This is marked, not in points and degrees, but with the initials and lines of direction of the known winds, and the idea was evidently taken from the wind cards or "wind rose," as it was called, which was in existence many years before the compass. Passing over a period of four hundred years, during which many developments in the compass took place, and coming to the early part of the nineteenth century, I find that at this epoch liquid compasses of various sorts were in more or less common use, and some of those used in H.M. Service are still preserved in the museum attached to the Compass Observatory at Deptford. Amongst them are two which have a particular interest in connection with the paper read this evening, in that the diameter of the card is considerably less than that of the bowl. In these compasses also the lubber line is removed from the bowl and placed near the edge of the card in the form of a vertical rod or wire, so that in this respect these compasses appear to be the ancestors of the compass described this evening. I may mention also that these compasses have an expanding bottom, evidently with the idea of making allowance for the change of volume of the liquid due to temperature. Some years later, in about 1879 and subsequent years, a well-known compass maker of Liverpool made and sold compasses in which the cards were considerably smaller than the bowl, and he also followed the earlier makers by fitting a vertical wire as a lubber line near the edge of the card instead of on the surface of the bowl. At least one of these compasses is in use at the present day.

Besides these compasses fitted with special lubber lines, there are many examples of compasses in which the diameter of the card is considerably less than that of the bowl, the amount of reduction necessary being dependent upon the strength of the needles to which the card is attached; that is to say, that the amount of drag or displacement of the card caused by the impetus given by the liquid owing to the turning of the bowl is inversely proportional to the strength of the needles. For instance, if no magnets were attached, the card would in most cases complete the revolution, only lagging behind the bowl. The strength of needles which, for practical purposes, can be used is governed by the necessity for avoiding induction in the soft iron correctors used in adjustment. The matter resolves itself into one of compromise, but I must express my surprise that this important point is not referred to in the paper.

Coming to the more immediate history of these compasses, it is true that compasses in which the diameter of the card is considerably less than that of the bowl have been in use in H.M. Service for some time. They are the outcome of compasses which were designed in the year 1901 for use in submarine boats. (The patent to which Mr. Dobbie refers was not published until March, 1902, and the main feature of the patent is a spring suspension.) Between 1901 and 1905 various experiments were made, and in 1906 extended trials were made afloat, as a result of which the compasses I refer to were adopted for general use in H.M. Navy in December, 1906, at which date I understand that Mr. Dobbie's compass was not in existence. Although, in common with their honourable ancestors, these compasses have (as is also the case in Mr. Dobbie's compass) a card of considerably smaller diameter than the bowl, they differ from them in one important particular. Instead of merely shifting the lubber line from the bowl towards the card, advantage was taken of the space which exists between the card and bowl to fit, instead of the vertical line, a horizontal pointer projecting from the bowl in the plane of the card, the extremity of the pointer being, of course, in close proximity to the card. Numerous advantages accrue from the use of this horizontal pointer, but as this matter is, in connection with certain Patent Office proceedings, still *sub judice*, I cannot go more fully into it than to say that one great advantage is that it enables a very convenient magnifier to be used, thus entirely overcoming the inconvenience which Mr. Dobbie tells us has been found in the use of his compasses. He states that he also provides a special lubber line or pointer. I should like to ask him if he will be so good as to describe the pointer he refers to. I hope that this history and explanation will have removed any doubt which may have existed as to the source from which the compasses in use in H.M. Service were derived.

Captain E. W. CREAK, R.N., F.R.S.: I am very glad that the important question of a suitable

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compass in fast modern ships has been brought before this Institution. It is important in two senses—(1) to show that a suitable all-round compass is available; (2) having the compass, to urge on naval architects the necessity of always providing a proper place for it in the ships they produce—*i.e.*, a magnetically proper place. I am much puzzled as to why for twenty years past so much ingenuity has been expended on nursing the dry compass by various kinds of suspension which have failed in turn, when an all-round instrument like a well-made liquid compass has been ready to hand. The best of modern dry compasses—Lord Kelvin's—has never succeeded in boats and yachts. In the early nineties it succumbed to the liquid compass in torpedo boats and destroyers, and now it has gone down before the latest pattern of that form of compass in the rest of the Royal Navy. The father of the present liquid compass was constructed in the early years of the last century by Crow, of Faversham, who took his idea from the skipper of a coasting craft. Being much troubled by an oscillating compass card in a sea way, the skipper found comfort when a sea broke into the binnacle and filled the compass bowl, thus steadying the card. In 1881 Captain Wille, of the Norwegian Navy, brought to my notice the value of preserving a considerable distance between the card and the side of the bowl. He also suggested replacing the usual lubber line on the side of the bowl by a vertical pin fixed in the bottom of the bowl and close to the card, as in Mr. Dobbie's compass. In 1889 I designed a liquid compass and azimuth circle, bringing all the latest improvements into its construction, the distance between the card and the bowl being increased from the usual 0·1 in. to 0·3 in. It was, however, declined by the Admiralty in favour of Lord Kelvin's compass, except for torpedo craft and boats and other special purposes where it has done good work up to the present. I would here pause a moment to pay tribute to the memory of Mr. Thomas Buckney, of the firm of Messrs. E. Dent & Co., to whose mechanical abilities I owed so much in the construction of this compass.

I am very pleased to note certain recent improvements in the liquid compasses made respectively under the direction of Captain Chetwynd, of the Admiralty, which have proved so successful in H.M. ships, and by Mr. Dobbie. The main feature of these improvements is a small card in a large bowl—in fact, carrying out what I had already begun in a less degree. The arrangements for lighting in the new Admiralty compass seem to be a great improvement, and the binnacle fittings are very convenient. Mr. Dobbie has made some kindly references to me and my work, and I hope he will not think it a bad return if I offer a few critical remarks on his paper. I differ from him as to who first suggested the use of compensating magnets. It was Sir George Airy, not Barlow, who invented a correcting disc of soft iron. Again, the sub-permanent magnetism of a ship's hull is developed by the action of the lines of the earth's magnetic force passing through the hull whilst running on a given course for some time subject to vibration from various sources, but only becoming apparent to the seaman when he alters course. Again, Lord Kelvin was not the first to introduce spheres for correcting quadrantal deviation. A pair of spheres were in use on board H.M.S. *Trident* in 1856, and are now in the Compass Museum at H.M. Compass Observatory, Deptford. I further submit that "hysteresis" is a fully descriptive term of what is called in the Admiralty books "sub-permanent magnetism." I agree with the author's objections to spring compensations. Lord Kelvin tried different forms of such suspensions in torpedo boats and destroyers, but they entirely failed. I am also in agreement with Mr. Dobbie as to resuming the original practice of Captain Flinders in keeping the Flinders bars separate from the binnacle. In modern ships these bars are the least successful of the correctors applied, especially in men-o'-war, where compasses are placed over conning towers and considerable magnetic induction is caused in the bars, of a kind injurious to their proper action as correctors. I must, however, differ with the author on the question of his removal of the points from his card. They should, I think, be subordinated to the degrees on the periphery of the card, but they are still wanted for registering the direction of the wind and other purposes. In conclusion, I would submit that the liquid compass is *the* compass of the present and near future. Personally, I have had no doubt about its superiority over the dry compass for the last twenty years. It is also a very economical compass—perhaps expensive at first cost, but remarkably durable. During the time I was Superintendent of Compasses at the Admiralty I have known several of those made by Messrs. E. Dent & Co. to be returned in excellent order after ten years' service; the exteriors showed

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signs of having lost their beauty, but the compass was absolutely sound. I trust that Mr. Dobbie will go on improving his compass, and I believe he has done good service to the seaman as well as the naval architect by the paper he has read to-night.

Mr. J. C. DOBBIE (Associate): My Lord and Gentlemen, I am very much gratified at the interesting discussion, and criticisms that have been made by Captains Chetwynd and Creak; but I may point out that my critics and myself agree on the main point, that a liquid compass in which the card is well withdrawn from the wall of the bowl, is the best form of compass. Captain Creak, as you know, is a very old Parliamentary hand on this subject, and I think he took part in the discussions in this Institution in former years.

Captain CREAK: I read a paper for Mr. Rundle in 1889,* as he was too ill to read it himself, and he asked me if I would read it for him, and I discussed the subject with Sir Philip Colomb, Admiral Fremantle, and others.

Mr. DOBBIE: As regards what Captain Creak has told us about others having gone in for this class of compass before, I am astonished that none of them thought of patenting their invention as I did mine in 1901; in fact, it is the first patent of the kind, and no one seemed to have thought it worth their while to do so previously. As to the favourable position for a compass, I am glad that Captain Creak agrees with me. In the old Allan Line steamers, they used to have a wooden mizzenmast with the compass on it. This mast was rigged entirely with hemp rigging and brass fittings, and in some cases there was absolutely no deviation from any cause whatever, either from hysteresis or from the magnetism of the ship's hull interfering with the force of the earth. Professor Ewing singles out the word "hysteresis" as being the best name, and perhaps I should have given it more proper prominence.

With regard to the Flinders bar becoming magnetised by other means than the semi-circular correcting magnets, that is so, and more particularly when the ship remains in one latitude for any length of time, but if she is going from North to South, the magnetisation by induction from the earth's force has been small, at least in my experience. (I am now speaking of the merchant navy.) As to the proposed card, here is a case where doctors differ, because some authorities on the subject to whom I have shown the card, with the degrees only, approve of it, while we have here two gentlemen who evidently think it is of no use, so I am afraid we must allow it to stay there in the meanwhile and not decide.

The PRESIDENT: I am sure we are all very much obliged to Mr. Dobbie for his most interesting paper. I used to know something about compasses myself long ago, and I could tell you one or two rather curious stories, but it is too late now. I think you will all agree with me in moving a hearty vote of thanks to Mr. Dobbie for his very interesting paper.

WRITTEN CONTRIBUTIONS TO THE DISCUSSION.

Mr. A. W. BAIRD sent the following written remarks: Mr. Dobbie's paper is open to criticism in respect of the totally inadequate reference he makes to Lord Kelvin's (Sir Wm. Thomson's) famous compass, and the complete departure which it marked from the ideas which had previously been held regarding the theory and practice of compass construction. An uninformed reader of the paper would be led to imagine that all Lord Kelvin did in his compass was to put into practical shape certain improvements which had already been thought out in theory by others. Those acquainted with the subject know how erroneous any such a supposition would be. Lord Kelvin's compass was radically different from any known compass which had either been conceived in theory or made in practice. The extreme lightness of the card and the extreme lightness (as well as shortness) of the needles were entirely the result of Lord Kelvin's mathematical and experimental researches into the complicated problem of designing a compass which would give efficient service on board iron vessels. Lord Kelvin subsequently

* Trans. I.N.A., Vol. XXX. p. 431.

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added to the new compass the grummet ring suspension, of which Mr. Dobbie speaks so highly, and also his well-known azimuth mirror. These were entirely new, both as regards conception and execution. Another point which Mr. Dobbie omits to make clear in his paper is the fact that the liquid compass, which has recently been adopted by the British Admiralty for service use, is that designed by Captain L. W. P. Chetwynd, R.N., Superintendent of Compasses. Mr. Dobbie states that in 1901 he took out a patent for the construction of a compass the card of which was considerably smaller in diameter than the inside of the compass bowl. If it had been worth anyone's trouble in 1901 to object to the sealing of Mr. Dobbie's patent, it would have been easy to point out that the advantages claimed were common knowledge, and that compasses of this description were made and used over twenty years ago. In fact, the writer had handled such compasses, and there are still some of these in service made by Cairns, of Liverpool, having a small card with vertical lubber line as described by Mr. Dobbie in his specification of 1901. So little, apparently, did the inventor—Mr. Dobbie—think of his own supposed new invention, that up to 1906, when Captain Chetwynd applied for his patent, he had never made one of these compasses, nor put them on the market. The compass now adopted by the British Admiralty referred to in Mr. Dobbie's paper is an improvement on Cairns', arrived at after many experiments by Captain Chetwynd, and exhibited by him at a conversazione of the Royal Society on May 8, 1907. It differs very considerably from Cairns', Creak's, and others made somewhere between 1815 and 1820; and, for vessels of high speed and excessive vibration, I have no doubt, will prove a great success. However, in vessels of normal speed and ordinary vibration it will be difficult to produce a compass so suitable as Lord Kelvin's standard dry compass.

Mr. Dobbie has sent the following additional note to his reply:—Captain Creak's explanations of the cause of the temporary magnetisation of the ship's hull is, of course, correct, and his statement amplifies my remarks. Some twenty years ago, I proposed to Lord Kelvin a rather complicated adjustment for this sub-permanent magnetism; but his opinion was that the value of this error is not constant, as it depends to some extent on the conditions of the moment. He illustrated his point by comparing the lapping of the ripples or waves on a ship's sides to hammers beating magnetism into the ship's hull, varying in intensity according to the prevailing conditions. Captain Chetwynd attributes to me the statement that smaller needles were employed because of the necessity of producing a light card, whereas I stated on page 292 that Kelvin's compass card was fitted with short needles to allow of the more perfect adjustment for quadrantal deviation, and I make a similar statement on page 294. In this connection I should mention that, as the influence of magnetism varies in intensity as the square of the distance, the *length* of the needles is the important factor, and not their *strength*, as Captain Chetwynd suggests. The same inaccuracy applies to his criticism of my remarks about sub-permanent magnetism, and his reference to semi-circular magnets. He mentions the difficulty I met with in reading the small compass card, but he omits to point out that I state I had got over this. My patent specification of 1901 (not 1902) makes it quite clear that this compass is a separate invention from the spring suspension Captain Chetwynd alludes to. In his application for a patent (referred to further on), he distinctly states that the compass card should be made of such less diameter than the bowl that it shall be substantially outside the range of the influence of the ring of damping liquid adhering to the bowl, and he mentions three-quarters as a convenient proportion. This makes it clear from his own statement that this element rules the strength and proportions of the needles used, and that there is no compromise between the two, as he now suggests. It was only in 1906 that the Admiralty made experiments with compasses which conform to that specification.

As to the source from which the compass in use in H.M. service was derived, Captain Chetwynd was given the opportunity by me of getting this compass made by my firm as far back as 1905, when I first discussed my invention with him. So unsuccessful, however, was Captain Chetwynd in producing a similar instrument of his own that did not trench on my patent, that I have now got a decision in my favour in my opposition to the sealing of his patent. At the hearing, Captain Chetwynd's representative abandoned any claim to the main invention, and the Comptroller General said that his invention was reduced to the substitution of a horizontal pointer for my vertical one, in the design of compass described and claimed

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by me in my patent specification; and he held that even this was included in my patent, and refused to seal a patent on the application. He differed from Captain Chetwynd as to the value of this horizontal pointer, and remarked that, on the evidence, any advantage gained was exceedingly problematical. I may say that I had tried and rejected this form of lubber line. He also remarked of the Cairns compass, which was produced as an exhibit, that here the card was so little withdrawn from the wall of the bowl as to raise the presumption that the purpose in view was to get clear of the shadow of the bizel for easier reading. He thus completely disposed of the claims made by my critics on behalf of the Cairns and other compasses. It is quite clear that in every compass a space must be left between the edge of the card and wall of the bowl, and so the card is always smaller than the bowl. No doubt some compasses have accidentally been fitted with smaller cards than usual; but no invention had been patented previous to my patent of 1901 setting forth as its object the withdrawing of the periphery of the card from the wall of the bowl, for the purpose of getting rid of the disturbing element of the annulus of liquid which more or less adheres to the wall of the bowl. Those interested will now know that the compass shown by Captain Chetwynd at the Royal Society Conversazione in 1907 was of my patent, fitted with Captain Chetwynd's invention, which the Comptroller of Patents has no hesitation in saying is covered by my patent. It is interesting to note that this compass has Mr. Baird's approval, and that he thinks it will be a great success. It is clear that if, as he is confident, it is a good compass for vessels under the most onerous conditions it will be even better, if possible, where the conditions are less onerous. It is difficult, therefore, to arrive at the grounds on which Mr. Baird bases his claims for Lord Kelvin's dry compass.

CONCLUDING PROCEEDINGS.

Mr. W. J. Luke (Member of Council): My Lord and Gentlemen, we have now reached the conclusion of our Spring Meetings, one of the most successful, I think, that it has ever been my good fortune to attend. I have been asked to propose one of the usual resolutions, that the best thanks of the Institution be voted to the Society of Arts for their kindness to us in letting us have the use of this hall. I think I am right in saying that the Institution has had its home here for the Annual Meetings practically from the time the Institution was formed, and that is very nearly fifty years ago. In this connection we are under a debt to the Society of Arts that is, I think, immeasurable. The service that they must have rendered to this Institution when it was young and struggling was a very great one indeed. Now that the Institution is strong and successful, and is able to run alone, perhaps we could, if it were absolutely necessary, go to some other premises; but so long as the Society of Arts are agreeable to having us here, I am sure there is not one of us but would very much regret if we were to leave this hall. Therefore, I will say no more, but propose that the Institution of Naval Architects express their very best thanks to the Society of Arts for their continued courtesy and kindness to us.

Mr. W. H. M. Ellis (Associate): My Lord and Gentlemen, I have great pleasure (and not for the first time) in taking part in this vote of thanks to the Society of Arts. We could not have a room better suited for our purposes. It is exactly our size, and it is close to our office, where we can get our papers. The Council of the Society of Arts deserve our most marked thanks, and I have great pleasure in seconding the resolution.

(The resolution was put to the meeting and carried unanimously.)

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Mr. R. A. HADFIELD (Associate): I have much pleasure in proposing a hearty vote of thanks to the Council of the Institution of Naval Architects for all they have done for us in the last few days. At this late hour of the evening, I am sure you will not expect me to speak at any length, but I may say that this is one of the most successful meetings I have ever attended in London, and I offer the Council my most hearty congratulations on the result.

Mr. F. EDWARDS (Member): I have much pleasure in seconding that.

(The resolution was put to the meeting and carried unanimously.)

Professor J. H. BILES, LL.D. (Vice-President): Gentlemen, there is, I am sure, nothing that an individual member of this Institution can say that can in any degree add to the very high opinion we all have of our noble President. He has been with us now for seven years, and we hope, though he has relinquished the office of President, that he will not infrequently sit in that chair and preside over some of our meetings. The duties he has had to perform have been arduous at times. I think the most arduous must be when he sits and hears discussions, long drawn out, in highly technical language, not even enlivened by the ferocity of anger or the brilliancy of wit, and he has sat and listened patiently to it all. I am sure you will agree to give him a most hearty vote of thanks for all the kindness to us in occupying his office, both inside the Council and outside in our public meetings, and for the splendid way in which he has always upheld the honour and dignity of the Institution.

Mr. W. H. WHITING (Member of Council): I have great pleasure in seconding this resolution. During the years that Lord Glasgow has presided over the destinies of the Institution of Naval Architects, the Institution has not only increased very largely in numbers, and advanced materially in regard to its financial resources, but it has increased in what is above all of importance, namely, its capacity for public usefulness. The two great industries of this country—shipbuilding and marine engineering—to promote whose interests this Institution was formed—owe, in this connection, no small debt to the wisdom, the judgment and kindness with which Lord Glasgow has for so long a period presided not merely over our public deliberations, but over the business of the Institution. It is with all sincerity that I second the vote of thanks to his lordship which Professor Biles has so well proposed, and which I am sure you will all agree to.

Professor BILES: I am sure I may say that that vote is passed unanimously, my Lord.

The PRESIDENT (the Right Hon. the Earl of GLASGOW, G.C.M.G., LL.D.): Gentlemen, I am extremely obliged to you all for the very hearty manner in which you have received the vote of thanks to me which has been proposed so kindly and well by my friend Professor Biles and seconded by Mr. Whiting. I can only say that it has been a great pleasure and interest to me for the last seven years to attend these meetings. Professor Biles has said that it must have been rather dull for me to listen to so much very technical detail. I must admit that most of the papers which I have heard read at these meetings have been very much over my head, but it has been a great pleasure to be present at meetings which in no remote degree concern the great profession in which I had the honour and pleasure of spending thirty years of my life. After all, if you cannot be at sea, I think it is very pleasant to listen to discussions about modern ships; it gives one a sort of scent of the salt water again, and for the remainder of my life I shall always look back to this period with very great pleasure. I thank you all once more very cordially for the kind reception which you have given me on this and all other occasions.

(The proceedings then terminated.)

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A CONTRIBUTION TO THE HISTORY OF IRONCLADS.*

INTRODUCTORY NOTE, BY THE RIGHT HON. THE EARL OF ROSSE, K.P., D.C.L., LL.D., F.R.S.

FINDING in the Journal of the Society of Arts, in the report of Sir William White's lectures of 1906, the statements that "the credit of initiating the construction of the earliest completed ironclads belongs to the Emperor Napoleon III.," and, again, that "these ironclads were floating batteries, built in 1854, and used during the Crimean War," it struck me that it might be of interest to look up old correspondence bearing upon the subject, which I felt sure would somewhat modify those statements.

I knew that my father had done what he could in conversation and by letter to urge our Naval and Military Authorities to take up the construction of armoured vessels for the attack on the Russian forts, and I understood that the part the Emperor Napoleon III. took was, on learning of the proposal, to press the matter forward, and get our authorities to take it in hand at once, when they were hesitating to move. Floating batteries were then put on the stocks, but were not, I believe, completed before the peace.

Were it not that the ancient history of ironclads has been brought into notice by being made the subject of the important course of lectures above referred to, I should have hesitated to put forward letters which, after the great advance of the applications of mechanical engineering to Naval purposes during the last half century, would seem quite out of date.

I remember the tracing referred to in Letter VIII. (February, 1855). My father thought the proportion of beam to length insufficient for combining the necessary resistance to penetration with buoyancy. He was also disappointed that the main structure of the hull was to be of wood.

The Howitzers referred to in Letter VII. (December, 1854) are still in my laboratory, and a punt-like model, 3 ft. 3 in. by 10 in. by 4 in., probably represents my father's general ideas as to proportional dimensions.

The letters have all been copied from my father's press-copy book, except the last two, which are from Wrottesley's "Life and Letters of Sir John Burgoyne," there being no copies in my father's book.

ROSSE.

Birr Castle, February 16, 1907.

LETTER I.

To SIR HOWARD DOUGLAS, BART., Author of "A Treatise on Naval Gunnery."

13, *Connaught Place, June* 11, 1854.

DEAR SIR HOWARD,—I return the pamphlet with many thanks, having read it with much interest. The principle laid down that assailable points should be guarded by forts, so as to leave the fleets free, appears to me to be but common sense. The Czar has acted upon it; we, I think, are not yet alive to it. As to the experiment relative to the resistance of plate iron, the thickness of the boiler plate is not given, nor the size of Commodore Stockton's gun. Boiler plate usually runs from $\frac{3}{8}$ in. to $\frac{5}{8}$ in. thick; taking it at $\frac{1}{2}$ in., the largest would have been $2\frac{1}{2}$ in. thick, while 5 in. seems the minimum for 32 lbs., so

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that if we add 2 in. as a reasonable margin, the largest should have been 7 in. thick, which, probably, would have withstood Commodore Stockton's gun. Even if the iron was to be 12 in., a properly proportioned vessel would carry the weight, even though it was not immensely large.

Believe me to be,

Dear Sir Howard,

Truly yours,

ROSSE.

LETTER II.

Letter from the EARL OF ROSSE to Sir JOHN BURGOYNE.

13, *Connaught Place, June* 12, 1854.

DEAR SIR JOHN,—You are probably worried with conundrums for destroying the Russian ships under their forts; this is mine. To build an iron screw steamer, proof against shot, shells, and boarders, and to run at the enemy's ship and sink it with one blow of the cutwater. To construct such a steamer I have no doubt is perfectly practicable; and I do not think it would be a very long business with the resources our great ship-builders have at command. The data we have would be sufficient to enable us to calculate the lines of the steamer, allowing, however, a very large margin of excess of strength: to prevent waste, a more accurate determination of the data would be necessary. First, the requisite thickness of iron. The best data I know of are the French experiments, detailed in the third edition of "Sir Howard Douglas on Naval Gunnery." From these experiments, I think we may assume that 3-in. iron plate would be shot-proof, except at a very short range, and where the impact was perpendicular; 4-in. plate, I think, would be safe under all circumstances which could occur in practice. The steamer should have no bulwarks, and when trimmed, the deck must not be more than 12 or 14 in. above the surface of the water. The first 2 ft. of the side would be 4 in. thick; the next foot, being considerably protected by the water, would be 3 in.; and the fourth foot 2 in.; the remainder of the hull ¾ or perhaps 1 in. thick. The draught of water would depend upon the size and shape, but with the machinery, and *without the deck*, it need not, I think, exceed 8 ft. As to the deck, I think it has been found that two layers of balk beams 12 in. thick each, crossed, is proof against heavy shells; I am speaking, however, only from a very distant recollection of the facts given either in the "Aide Mémoire," "Jones' Sieges," or the "Professional Papers." I suppose that to be so; then 2-in. iron plate would be about an equivalent; and as iron is nearly eight times as heavy as water, a deck of 2-in. iron plate would increase the draught of water 16 in., so that, allowing for deck beams and fastenings, the total draught might be, perhaps, 10 ft. Before the lines of such a steamer were calculated, the best data should be obtained.

(1) I think it would be desirable to repeat the French experiments, employing a succession of plates riveted together, which is the way the required thickness would probably be obtained in practice. (2) To obtain the best information as to the impact of heavy shells. (3) The amount of protection which might be relied upon from the water at different depths. Should it be found that greater strength would be required, either in the deck or sides, it would only be necessary to increase the size: the great principle that the buoyancy increases much more rapidly than the surface would make all difficulty surmountable. The funnel need not appear above deck, as a fan would answer instead, and the hatchway could be effectually secured against boarders, the vessel being steered below. Small holes for muskets would probably be sufficient to keep the deck clear and prevent grappling.

Ships in docks would, of course, be safe, but I think nowhere else where there was a fleet outside. You may, perhaps, discover some fatal objection to all this, but if not, perhaps it might be worth considering.

Believe me, &c.,

In the rough calculation I have assumed 300 as the nominal horse-power. ROSSE.

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LETTER III.

To SIR JOHN BURGOYNE, BART.

13, *Connaught Place, June* 26, 1854.

DEAR SIR JOHN,—As a little supplement to my former hints, I will add that I have roughly examined the question whether it would be practicable to construct a vessel proof against shot and shells capable of carrying an efficient breaching battery, and yet not so large as to make the expense a fatal objection. The result is that I think the project quite feasible. As such a vessel would probably be employed in water nearly smooth, I think she need not carry her guns more than 3 ft. above the water. The exposed sides of the vessel would be of plate iron 5 in. thick, and her ports might be round and a little larger in diameter than the guns. Her deck would be about 6 ft. above the water, and 2 in. thick of plate iron. No bulwarks. Probably you would consider sixteen heavy guns sufficient, and that eight guns at fifty yards would in a reasonable time effect a breach in one of the Cronstadt forts. Such a vessel need not draw more than 12 or 13 ft., and, if the little charts in the booksellers' shops are trustworthy, there is more than that depth of water close to the forts. About 1,500 tons would, I think, be large enough, with a beam excessive in proportion to her length, to give the necessary buoyancy and stability under top weight. All this is the roughest possible, but I think if worked out in detail the result would not be widely different. The greatest care would, of course, be necessary to guard against submarine explosives. Should the Russians cast a few enormous guns to meet the attack of such a ship, I should think, by a little management, it would be possible to take up a position out of the line of their fire, and while taking up that position so to steer as to prevent the ship from being struck, except at a very oblique angle.

I have quietly spoken to several of our Fellows, three of them civil engineers, and one an artillery officer, and I have not heard any doubt expressed as to the practicability of effecting the object in the way I have suggested.

Believe me to be,

Dear Sir John,

Yours truly,

ROSSE.

LETTER IV.

From SIR JOHN BURGOYNE, BART.

Ordnance Office, Pall Mall, July 2, 1854.

MY DEAR LORD ROSSE,—I have many apologies to make for not having answered your two letters earlier, but as they are on very interesting subjects, I was in hopes before I replied to them to have obtained more information than I possess on the matter, and that, after all, I have not been enabled to do, but prefer writing in an unsatisfactory manner rather than appear to be guilty of neglect.

I presume that the application which your Lordship proposes for your naval construction of vessels is rather to show the value of the idea should it prove to be well founded—than with the intention of entering into much discussion about the particular modus operandi; indeed, as the whole depends upon the principle of the shot and shell proof vessel, the real question is to be establish the practicability of that principle in the first instance, and ascertain the data necessary for that object before drawing on discussion as to the manner of applying it.

If one could obtain shot and shell proof surfaces by any at all moderate degree of substance, weight and expense, it might be turned to very useful account by land and sea in various ways. I do not know on what ground you conceive that iron of 5 in. thick (*sic*) would answer, but my impression is that against

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a 32-pounder at a high velocity and vertical impact, however manufactured, it would be thoroughly shattered by even a single shot supposing the surface to be considerable, such as the side of a ship without cross supports within. Iron is very treacherous, and breaks, rends and tears under very irregular efforts. The Navy have a thorough dislike to it for the sides of ships, but then they have never contemplated, I believe, such thickness. There is then the question of the effect when struck at different angles. By trials against massive stone walls, the shots glanced off up to the wall being at an angle of about 77 deg., after which the shot has a tendency to penetrate, but it is a curious circumstance that in every instance of the shot glancing off the wall, it (the shot) invariably broke in two or more pieces. The shot, however, was cast, whereas your walls should be, of course, malleable or rolled. One matter of experience in favour of your system would be that a ship cannot be struck with any force by a shot more than 2 ft. under water, if so much. The only trials of shot-proof sides for vessels that I am aware of was for the floating batteries to breach Gibraltar in 1782. D'Argon, the French engineer who invented them, wrote an apology for their want of success: it is a very rare work, I have never been able to see it, but I understand that he had a close network of iron, which was some feet deep, and the square chequered interstices which were not above 1 in. or 2 in. square, filled with hard wood. He said that, by experiments at Cadiz, it was proved to be shot proof, and that, in the end, they were not set on fire by the red hot shot, but by their crews. With respect to your decks, plunging shot and shells may hit them very hard—but if two or three men of such knowledge as your Lordship, and those you mention, conceive the success even *probable*, most decidedly it ought to be tried, but first at a target before putting it in the form of a ship.

Yours very truly,

JOHN BURGOYNE.

LETTER V.

To SIR JOHN BURGOYNE, BART.

Athenæum, July 3, 1854.

DEAR SIR JOHN,—The grounds upon which I took 5 in. as sufficient were the French experiments at Metz, given in the third edition of Sir Howard Douglas' "Naval Gunnery," page 164. It is there stated that a 24-pounder with a charge of 4 lbs. 4 oz. at 66 ft. distance did not pierce a plate of iron 3·08 in. thick; with 6 lbs. it did so. From this some estimate may be made, though a very rough one, of the thickness necessary to resist a 32-lb. shot with a full charge, and it accords pretty well with deductions from theory. It appears that iron $\frac{5}{8}$ in. thick almost breaks the shot to pieces (page 167), so that the shot does not act upon very thick iron plates by virtue of its form, but mainly by its momentum. The power of resistance of iron plates under such circumstances would increase much faster than their thickness. This kind of reasoning is no doubt very unsatisfactory in comparison with experiments, and the only inference which at present I should venture to draw is that there is a certain thickness of plate iron which would resist 32-lb. shot, and that a ship of no very great size would be able to carry it, provided it was properly shaped and very low in the water. At one of the Royal Society soirées a 9 or 12 lb. shot was exhibited, which had been laid upon a block of Indian rubber and struck with Nasmyth's hammer. The shot was partially flattened and fissured all round the edge, just as if on the point of going to pieces; the Indian rubber was uninjured. Here we see the cohesion of the shot was not sufficient to bear the strain of being forced into the Indian rubber; had the shot been projected, even with a small velocity, its *momentum* no doubt would have carried it through.

Believe me to be,

Dear Sir John,

Very truly yours,

ROSSE.

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LETTER VI.

To the DUKE OF NEWCASTLE.

July 6, 1854.

Lord Rosse presents his compliments to the Duke of Newcastle, and begs to say that he has been considering, no doubt in common with many others, in what way the great mechanical resources of England could be brought to bear against the mechanical defences of St. Petersburg. The records of sieges show beyond question that a few heavy guns within a range of 50 or 100 yards will soon demolish the strongest mason work. The question therefore is, could a floating battery be constructed proof against shot and shells and yet sufficiently buoyant to carry a few heavy guns with the necessary supply of ammunition? This appears to be quite practicable. First as to the material, the Spaniards tried wood against Gibraltar and failed. Their floating batteries were destroyed by red hot shot that now, although by modern processes wood can be rendered in some measure fireproof, still modern gunnery would destroy wooden batteries however strong, as shells fired horizontally would lodge, and bursting would soon blow them to pieces. Plate iron appears to be the suitable material. The next question then is how thick the plate iron should be. No doubt there is a certain thickness which would be quite effectual. Strange as it may appear, no experiments seem ever to have been tried either by our naval or our military authorities on the resistance of plate iron to artillery. Recently, indeed, experiments have been tried at Portsmouth on the splintering effects of shot passing through thin plate iron. These experiments were tried *after* several war steamers had been built of thin plate iron, and the result was the steamers were condemned for war purposes. Strange to say the authorities were then satisfied and proceeded no further with the experiments, instead of completing the inquiry by investigating the other case, the effect of shot on thick plate iron of sufficient substance to stop the shot altogether. The only experiments of the kind have been made at Metz, and the results are given in the third edition of Sir Howard Douglas' "Naval Gunnery," but they were made with a different object, and are quite insufficient as data for the purpose of computing the tonnage and proportions of iron floating batteries. As far as can be inferred from the French experiments (but it would be unsafe to trust to theoretical deductions from such imperfect data), the minimum thickness of plate iron, or rather a series of plates of iron, to resist the direct fire of 32-lb. shot should be 5 in., and to resist the descent of heavy shells it should not be less than 2 in. Upon these data Lord Rosse computed in the roughest manner the proportions of two screw steamers, one *without* armament but with considerable steam power which might enter a harbour however guarded by batteries, and running full speed at any ship at anchor might sink it with one blow of its cutwater; the other, a steamer with small steam power but with a few heavy guns under a bomb-proof deck to be laid as close as possible to the fort to be breached. He sent the results of these very rough computations to Sir John Burgoyne about a month ago. Sir John Burgoyne very properly observes that till the effects of shot upon plate iron shall have been better ascertained, it would be impossible to form any accurate opinion, but that the subject is one of much importance. Should the Government think it advisable to direct the authorities at Portsmouth to make the necessary experiments, a target could be put together in a few days, and the preliminary experiments might be completed in a week. How far the experiments should be carried would depend very much upon the first trials. The data having been accurately ascertained any of our leading engineers would have no difficulty in computing the size, proportions, and probable cost of a shot-proof steamer suited to the required purpose. Lord Rosse has mentioned this matter to a few of the Fellows of the Royal Society best acquainted with such subjects, and no serious difficulty has been suggested; he has refrained, however, from mentioning it publicly.

LETTER VII.

To SIR BALDWIN WALKER.

Castle, Parsonstown, December 16, 1854.

DEAR SIR BALDWIN,—I am sure you will think it rather impertinent in a landsman to give you a hint, but I cannot refrain from doing so. Last spring, having ascertained by computation on ordinary

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mechanical principles within small limits of error the thickness of plate iron which would resist 32-lb. shot under various circumstances, a very easy matter with the excellent modern engineering data we have; and having in some degree verified the results by comparing them with deductions (?) from the French experiments, I computed roughly the proportions of shot-proof ships which, with the necessary strength, would have the necessary buoyancy, and I sent the result to Sir John Burgoyne. A difficulty stared me in the face *in limine*, that such vessels though without masts might be liable to capsize in a heavy sea as there was so much top weight; but a little simple calculation made it evident that the required stability could be obtained without sacrificing essentials, and I did not think it necessary to allude to that point in writing to Sir John Burgoyne. It now occurs to me *barely possible* that in constructing your shot-proof ships there may be the oversight of omitting to make the necessary provision for stability in a heavy sea, in which case there might be a serious disaster. The required calculations are perfectly simple when the data are determined, but simple things are sometimes overlooked, and it is quite possible that a ship apparently stable in smooth water might in a heavy sea turn right over. I have therefore ventured to throw out this hint, as perhaps you might think it expedient to mention the subject to your naval architect.

There is another point. In writing to Sir John Burgoyne I assumed that the projectiles to be resisted were 32-lb. spherical shot, and on that assumption I obtained 5 in. as the minimum thickness at a perpendicular incidence and at very short ranges. At that time (the first week in June) heavy rifled ordnance had not, as far as I was aware of, been employed with much success. Now it seems to be otherwise, and the ships, or rather floating batteries, may find at Cronstadt antagonists far more formidable than the ordinary 32-pounder. Of Mr. Lancaster's rifled ordnance I know nothing practically; I have never seen them. The contrivance appears to me to be very unmechanical, and I should not be surprised if the Czar has something very much better in spring. The scientific Russians I have occasionally been thrown in contact with have always represented the Czar as a very able man thoroughly conversant with everything relating to the *matériel* of war. He has a magnificent foundry with every mechanical appliance. He has Jacobi and other ingenious and able men at his elbow. It is very probable they will suggest to him that to sink your floating batteries as described in the newspapers, it is only necessary to cast heavy rifled ordnance. This he can do with great facility. In the present advanced state of mechanical engineering guns can be cast hollow, sound, and smooth enough for immediate use. Woolwich people would perhaps smile at this, but it is nevertheless the fact. Four or five years ago, requiring a few iron guns for some experiments, they were cast in my laboratory: we cast them hollow, and they might have been at once proved; but, as I wished the bore to be quite smooth, a cutter was passed through. The surface of the bore was free from specks, and the iron as sound as if the guns had been cast solid: the largest was but an 18-lb. Howitzer gun, but there was nothing to prevent their being cast of any size. The cylinders of the largest steam engines are every day cast in the foundries of England and Scotland perfectly sound and free from specks. Instead of the core being round, it might have had the oval twist of Lancaster; or, what I should have thought much better, it might have been formed so that the gun should have had two *rectangular* broad grooves to receive long *rectangular* prominences on the sides of the elongated projectile. There would be no difficulty in making a very accurate metallic core-box to produce a gun rifled on any system; little more would be required than the slide-lathe and planing machine. The Czar's foundry would no doubt turn out a very heavy armament of rifled ordnance in this way, and that in a very short time.

Supposing a rifled 32-pounder strong enough to project an elongated shot of, say, 50 lbs., with the ordinary initial velocity of a round shot, it would strike the mark if not quite close with a velocity exceeding that of the 32-lb. spherical shot, and therefore its momentum would exceed that of the 32-lb. shot more than in the proportion of 50 to 32. Such a gun would, I think, at 300 yards, easily pierce 4-in. plate. It is impossible to calculate very exactly the additional thickness which would be required to resist it, but $1\frac{1}{2}$ in. would, I think, be barely sufficient. You may be opposed, however, by more powerful guns.

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If the newspapers are correct and your vessels are to be of 2,000 tons burden, there is an ample margin of buoyancy, and, if difficult to add iron, perhaps oak beams would answer as a lining. Shells would not pierce the iron plates because the momentum would be diffused over so large a space, in fact there would be so large a piece to be punched out; besides, the effective momentum would be less, or there would be two impacts, one from the materials of the preceding hemisphere of the shell, the other from the materials of the following hemisphere. Elongated shot, slightly red hot, would, I think, pass through, but whether there would be anything to be apprehended from their lodging in the oak you know much better than I do. Upon this subject, of course, I feel a great interest; but, if possible, I feel in addition a *personal* interest. The beginning of July I wrote to the Duke of Newcastle drawing his attention to my letters to Sir John Burgoyne, and stating to him that Sir John so far agreed with me that he was of opinion that an iron target should at once be made. I mentioned to him at the same time that the preliminary experiments might be made in a week. What the Duke did I know not, but, hearing nothing, and thinking that nothing would be done, at length I sent for iron to make a target, and it had scarcely arrived when I saw in the papers the account of your experiments at Portsmouth. Before I wrote to Sir John Burgoyne I conversed with a few eminent engineers to ascertain whether they could start any insurmountable difficulty, and I found they could not. I scarcely spoke to anyone else, still I am sure if anything went wrong my name would be brought out in connection with it. It is, therefore, that I say I am personally interested. Having so far as was in my power put you on your guard, I do not wish to give you the trouble of writing to me, knowing how busy you are; but, should you write, do not tell me any State secrets. If you did, I should be tongue-tied in the House of Lords, where, possibly hereafter, I may find some opportunity of being of use.

Believe me to be,

Dear Sir Baldwin,

Very truly yours,

Sir Baldwin Walker. (Signed) Rosse.

Letter VIII.

To Sir Baldwin Walker.

Castle, Parsonstown, February 23, 1855.

Dear Sir Baldwin,—I return the trace with many thanks. With your light deck the question of stability appears to me to present no difficulties. I perceive, however, in the new edition of Sir Howard Douglas's book, which I have just received, that he expresses doubts upon the subject; however, on looking into the matter after refreshing my memory from my old College book, Poisson's "Mechanics," I do not see any grounds for Sir Howard's apprehensions. The centre of gravity is high, but the metacentre is much higher, owing to the form of the ship. The armament will, of course, raise the centre of gravity, but I presume the engine and stores will produce nearly an equal effect, and in the opposite direction. If, indeed, there were masts of any considerable weight, and a great wave was to break upon the deck, retained for a moment by the bulwarks, while another wave threw the vessel over on the side, it is possible that the position of unstable equilibrium might be reached, and the vessel turn right over; therefore I should think it would be unwise to employ masts and sails except the lightest possible, and it would obviously be prudent to provide the most ample means for the instantaneous escape of water from the deck. In the case I calculated roughly last spring, the deck was to be very heavy, proof against shells *thrown from mortars*, and plunging fire of any kind, and there some care would have been required to provide for the necessary stability. As to strength, I fear, in this respect, you will not find the floating battery all you could wish. From the account Sir Howard Douglas gives of the effect of shot on granite at Romarsund, it seems almost a waste of ammunition to fire at a greater range than

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400 yards, and, at that distance, I do not think the sides of the floating batteries will withstand the heavy solid shot guns with which it is said the Russians are amply provided, to say nothing of the rifling or other contrivances for projecting elongated shot. The plates, I fear, are of insufficient strength, and at the uncovered butt joints the construction is obviously weak: a shot striking within 3 or 4 in. of a joint would find a rent made for it which it would have to make were it to strike the solid plate; and at the angle of a plate it would encounter still less resistance. To strengthen the sides materially with the present form and displacement would, I apprehend, be impracticable. I do not, however, see any reason why the bow of each boat might not be fortified by an additional layer of plates, or by other means, so as to make that part at least secure. There two of the heaviest solid shot guns might be placed, and as there will be five floating batteries, there would be ten guns to effect a breach; an ample number, I should think, judging from the data in "Jones' Sieges," and in the "Aide Mémoire." With the bow directed to the enemy's guns, the deck, no doubt, would be much exposed to plunging fire; but something considerable might be gained by loading the vessel at the stern so as to diminish the angle at which the projectile would strike. We have no data I am aware of sufficient to make it practicable to calculate at what angle a shot of a given size, and with a given velocity, would penetrate a wooden deck 8 in. thick. The yielding nature of the material would affect the result, and I think considerably in favour of the resistance offered by the deck. The timber would be furrowed, and the direction of the shot changed gradually, so that the deck would not at any one moment be exposed to the same violent strain as if the direction of the shot was changed *per saltum*. I do not recollect to have seen any attempt to investigate the effect of shot on timber at varying angles, either in the "Aide Mémoire," gunnery books, or the treatises on mechanics, but, as far as I can judge, I do not think there is much reason to apprehend that the deck will be penetrated by plunging fire unless the angle is considerable: it will be injured whenever struck, but I presume there will be with the plant an ample supply of planks, and spikes, to make defects good at once.

I need not add that, of course, heavy shells *thrown from mortars* will penetrate such a deck, and perhaps pass through everything; but you know better than I do what chance there may be of the deck being so struck. Mortars can be so easily cast, ready for use the moment the vent is drilled, that the Czar may have provided an immense number of them. Pray excuse these trifling remarks: had I been equal to outdoor work, which is not yet the case, I should have been glad to have tried a few experiments so as to have been able to speak somewhat less vaguely as to the endurance of your decks under plunging fire.

Believe me to be,

Dear Sir Baldwin,

Yours truly,

ROSSE.

LETTER IX.

TO SIR JOHN BURGOYNE.

14, *Adelaide Crescent, Brighton, September* 15, 1865.

DEAR SIR JOHN,—You have so often reminded me of our correspondence at the beginning of the Russian war, about the project of casing ships with iron, that perhaps it may interest you to know the sequel. As I thought that nothing but iron could cope with granite, and as you did not suggest any insurmountable difficulties; as, moreover, the engineers I had an opportunity of speaking to, however sceptical at first, gradually seemed to come round, I thought it might be useful to give a hint on the subject to the authorities. Mr. Nassau Senior was a great friend of Lord Lansdowne, who was a member

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of the Government. Moreover, Lord Lansdowne had not the same prejudices against science and everything belonging to it, as official personages usually had at that time, who believed nothing which was not the result of pure practice. It appeared to me that it might be well to mention the subject to Mr. Nassau Senior, with the view to his drawing Lord Lansdowne's attention to it. Mr. Senior at once entered into the subject with his usual energy; but he did not think that Lord Lansdowne, at his age, and without professional prestige, would be able to effect anything. He advised me to raise a discussion in the House of Lords; my objection, however, to that was, I should only give the Russians notice to prepare bigger guns. He said the next best thing would be to communicate with the Duke of Newcastle, who, though unacquainted with such subjects, was a man of great energy, and that he perhaps would put some pressure on the Lords of the Admiralty; that the Lords of the Admiralty, though thoroughly conversant with routine business, had not the general knowledge necessary to enable them to deal with new subjects; and that if I wrote to them, they would merely throw the letter into the waste-paper basket. I wrote, accordingly, to the Duke of Newcastle, stating that I had discussed the question with the most competent men, and that I had no doubt as to the results of calculation: first, that 5-in. plates would be amply sufficient to cope with the Russian guns, and, second, that vessels of no very great size would safely carry them. The Duke, no doubt, sent my letter to the Admiralty, and, I presume, it speedily found its way into the waste-paper basket. It would have been wiser, as it turned out, to have acted on Senior's first suggestion, and, through the House of Lords, to have endeavoured to stir up the officials, and to have called upon the Admiralty to give reasons for their inaction. Vessels might have been constructed in time, which would have entered Sebastopol, and taken the batteries in reverse. As to the future, you seem to think the guns will beat the ships. There is a limit, however, in the nature of things, to the power of guns, and that limit, I think, will be reached sooner than many expect.

The last time Mr. Nassau Senior was with us, we talked these questions over. He was a man of very clear mind, but not a mathematician or mechanic. He was anxious to obtain information for his friend Sir G. Lewis. The question seemed to me to take this form: What is the most powerful gun which men will be able to construct of existing material? Certainly a very general question. A formula sufficient to answer that question under all circumstances was soon obtained. The first result was that the projectile should be spherical; then, given the minimum initial velocity which could give a sufficiently good trajectory, and given the pressure per square inch within the interior of the chamber which the material of the gun could bear without being crushed, it was easy to calculate the diameter of the largest gun which would stand. Before I gave a statement of the principle to Mr. Senior, I submitted the investigation to Mr. Purser, a very able mathematician, now Professor of Mathematics in the Queen's University, then private tutor to my son. He had the subject a week before him, and then gave in his full adhesion. I can have no doubt, therefore, that all is right. Indeed, from what I read of the Shoeburyness experiments, I believe practical men are beginning to feel what calculation had predicted, that we are approaching the limits which the nature of material has set to the power of guns. As to ships, provided that no greater height above water is insisted upon than that of the *Scorpion* or *Wyvern*, I think that we may easily plate them so as to defy the largest guns. We must have beam in proportion to the thickness of the plate, and we must have great length, if we require much speed, but not otherwise. I have great faith in the power of calculation, if we proceed cautiously; and I believe there is but little of the information which was obtained at Shoeburyness, which might not have been previously worked out on purely theoretical considerations. I met Whitworth at Portsmouth, during the visit of the French fleet. He said they had now a prospect of obtaining better material for guns, that may perhaps extend a little the boundary line between the possible and the impossible; but it can in no way invalidate the main result to which calculation points.

Believe me to be, dear Sir John,

Truly yours,

ROSSE.

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Letter X.

From Sir John Burgoyne.

Brighton, September 16, 1865.

My Dear Lord Rosse,—You have evidently given much consideration to the very arduous problems connected with armour-plating for men-of-war; and I know no one better able to form decisive judgments on them than one of your scientific, mechanical, and searching mind. To me, however, it is still full of difficulties, which I am quite unable to reconcile. First, to provide a covering that should be shot proof, entailing such a great additional weight to the structure of the ship as shall much lessen the capability of carrying its other necessary appurtenances of guns, ammunition, fuel, provisions, and stores, &c. Then the distribution of that extra weight round its contour, so different from the principles of diffusing a load for a good sea-going ship, hitherto thought best, are problems for nautical consideration that do not appear to be solved up to this time, except perhaps partially by having ships of enormous size.

Then you rely much more than I do on the power of the 4½-in. or 5-in. plating, backed with timber, to resist shots from guns that there is no doubt can be easily manufactured. What is called invulnerable is that which is not perforated by a single or a few dispersed shot; but *my* requirement would be the withstanding a degree of *battering* by a number.

I do not presume to take up the subject of the contest of ship against ship in all its phases, which I leave to naval men; my reflections have been turned entirely to shore batteries as opposed to ironclads, and in particular to the defence of entrances to harbours, to rivers, &c., of moderate width—say, where the channels for the vessels do not exceed from 1,000 to 1,500 yards from the shore.

On the land, then, there will be probably 20 to 200 guns of the best of the day (according to the importance of the station), bearing on the approach, and on the passage itself; they will be dispersed if possible, and the more they can be on elevated sites, up to 80 or 100 ft. above the water, the better, as the decks of the vessels, which are imperfectly protected, will then be more exposed than the sides. As the vessels approach end on, they will afford a considerable and very fair target to the batteries, even at great distances; the part exposed being the decks, even from low batteries, the firing from which will be at an elevation. When entering the passage, every shot *must* tell; for as the ship cannot run its own length during the time of the shot's flight—the guns being all laid on a concentrated part, and point blank, and at a well-known elevation, and fired when the bow is on the line of sight—she must be hit by every shot; and if fired by signal in salvos, it may be conceived what will be the effect.

With regard to the fire of the ironclads on the batteries, it may have a powerful effect upon one of exposed masonry, insulated and compact, when opposed for a considerable time to dispersed vessels with powerful guns; but where the shore batteries can be more or less separated in distance and height, with no exposure but of earthen parapets, or rather of their merlons and embrasures, the scattered shot directed on them in action will be thoroughly ineffective. Thus, instead of the heavy broadsides of the old men-of-war, that overwhelmed any insulated battery that might be nearly the same level, and with which it could at all close, the effect of the three or four guns, however heavy, of the ironclads, will make but little impression.

As to the invulnerability of the iron plating, it has been tried in the way most favourable to it—that is, on targets made with peculiar care, expressly for the trial, every joint and bolt newly applied and fastened, and without being previously subject to wear and tear, to the violent strains of working at sea, or to partial concussions; and yet the 4½ in., with its backing, has been thoroughly perforated at 200 yards range, with a Whitworth 70-pounder. It was also to be observed that, where a heavy shot made only an indent of an inch or two on the surface, the shock had driven in bolts and splinters from the timber lining within, precisely on the principle that, in the game of croquet, a ball held down to the ground

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firmly with the foot, and struck with violence, will drive another that is in contact with it, but loose, to a considerable distance. Thus it may be inferred that a number of hard blows from heavy shot, without absolutely penetrating, will disintegrate the entire mass, and reduce the whole to a great state of weakness.

Nor is it necessary, as I conceive, to have recourse to the enormous class of guns that are from time to time progressively suggested. It appears to me that it would be most advantageous to employ the smallest guns that will really make effective impression, and to multiply them, in preference to having a small number of the monsters. I conceive that a piece carrying a shot of 100 to 150 lb. would have sufficient effect for the purpose.

That such guns, having a power of lancing their projectiles with high initial velocity and accuracy, as well as being thoroughly durable, might be readily manufactured, there can be no doubt; they would be, of course, much more easily transported, handled, and served, and by their increased numbers would have fully as great an effect—on the principle that it has been found by experiment, that two 12 or 18-pounders would breach a stone wall with the same aggregate weight of shot as effectually as one 24 or 36-pounder. I have propounded this principle as worthy of consideration, under the difficulty of manufacturing and serving enormous 300 and 600-pounders; but the suggestion has been rejected by the Ordnance Select Committee.

Lastly, as against the power of ironclads to force entrances into harbours or other restricted passages, we have the system of torpedoes to have recourse to, which may be even rapidly improvised to a great extent with very moderate means, and with which it would be most difficult to contend.

While I argue somewhat strongly on the difficulties in the way of the ironclads performing all the great services contemplated by many enthusiasts in their favour, I am quite sensible of certain advantages they possess as opposed to other ships, which render it impossible for us not to adopt them if other maritime powers do, and not to strive how in the greatest degree to remedy the many disadvantages which as yet attend those of all countries that have studied and tried them.

My dear Lord Rosse,

Yours faithfully,

J. F. Burgoyne.

Further references to the subject: "Life and Correspondence of Sir J. Burgoyne," by Wrottesley, Vol. ii. pp. 354 to 359. Also, Correspondence between Sir John Burgoyne and I. K. Brunel on armour-plating, shore batteries, &c.

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OBITUARY NOTICES.

THE MARQUESS OF LINLITHGOW, P.C., K.T., G.C.M.G., G.C.V.O.

FEW public men of our day have been more deeply and sincerely regretted than the late Lord Linlithgow. He was known and admired by a large circle of friends both in these Islands and in Australia, but by no section of his fellow-countrymen was he held in greater esteem than by the members of this Institution.

When Lord Brassey retired from his brief Presidency in 1895 in order to serve his country as Governor of Victoria, Lord Hopetoun, as he then was, had just returned to Great Britain after filling the same Governorship for five years, to the very great satisfaction of our Australian fellow-subjects. His Lordship was known to have taken from his youth the deepest interest in everything that concerned ships and sailors. His reputation as a wise and tactful administrator had followed him home, and the Council unanimously resolved to invite him to become President of the Institution. He accepted the invitation, and for a little over five years he filled this office to the great advantage of the Institution.

The period of his Presidency was one of the most eventful eras in the history of the Institution. In the year following his election he was called upon to lead the members in their memorable visit to Hamburg and Berlin, and he rapidly gave proof of exactly those qualities which were needed to ensure the success of the meetings. His speeches were admirable, both in matter and manner of delivery; his tact was unfailing, and his courtesy and social charm so great that all who came in contact with him, either members of the Institution, or their German hosts, felt that they had in him a personal friend.

The year following (1897) brought fresh labours and fresh triumphs to Lord Hopetoun. It was the year of the late Queen's Diamond Jubilee, and the Council resolved to celebrate that event, and at the same time to show their gratitude to their French and German friends, by holding an International Congress of Naval Architects and Marine Engineers in this country. His present Majesty, who was then Prince of Wales, graciously consented to become Honorary President of the Congress, and inaugurated the meetings in the Great Hall of the Imperial Institute. Lord Hopetoun was asked to accept the active Presidency, which he consented to do, and devoted

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himself unsparingly to the arduous preparations and to presiding at the various meetings and entertainments. The impression which the President produced on the visitors and guests of the Congress is recorded in the numerous addresses which were afterwards presented to the Council by the foreign countries and societies represented. The members of the Institution, as a slight token of their gratitude, asked their President to accept two silver loving cups in memory of the occasion.

Lord Hopetoun continued to preside over the Spring Meetings for three more years, but he was only destined to conduct one more of those Summer Meetings which had contributed so much to furthering the prosperity of the Institution. This was the North-East Coast Meeting of 1899. Before another Summer Meeting was held Lord Hopetoun was called away to a wider sphere of activity, and was compelled to resign the Presidency of the Institution.

In the year 1900 the Act which federated the various Australian Colonies and Tasmania became law, and Lord Salisbury's Government, interpreting the unanimous wish of the Australian Colonies, which completely accorded with their own views, recommended Queen Victoria to appoint Lord Hopetoun as the first Governor-General of the new Commonwealth. In the month of October of that year he left England in order to inaugurate the opening Parliament of United Australia, which it had been arranged should meet on the first day of the new century. He carried with him the good wishes of every member of the Institution. Before starting, his Lordship and Lady Hopetoun were entertained by the Institution at a farewell dinner, at which an address of congratulation and of leave-taking was presented to him. The affection and esteem in which he was universally held, and the record of his great services as President, were aptly expressed in this address.*

Before passing on to other portions of Lord Hopetoun's career it should be mentioned that he was never physically a strong man, and his duties as President were often discharged with great difficulty when his health was in a serious condition. The labours attending the Congress of 1897 almost broke him down. Few of those who have listened to his admirably lucid and perfectly delivered addresses would have suspected that he had an unconquerable horror of speech-making. Nevertheless, this was the fact. The preparation of his speeches he took most seriously, and brought to bear on them all the powers of his mind, but the anticipation of having to deliver them tended to aggravate his physical condition.

To the members of this Institution that part of Lord Linlithgow's career which was connected with its affairs is, naturally, of special importance; but he was a man of many parts and of great activities, and the rôle which he played in this Institution, though a matter of great interest to himself, was only a transient feature in a very busy life. He was known as a Colonial Administrator, a Cabinet Minister, a high Court functionary, a great landlord, whose interests were identified not only with

* Trans. I.N.A., Vol. XLIII., page xxxi.

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agriculture, but with one of the leading industries of his native land, and, lastly, as a most ardent sportsman. The details of his public career are well known, and need only be referred to here for the sake of record.

He was born in the year 1860, and succeeded his father as 7th Earl of Hopetoun in 1873, when he was still a boy at Eton. Ten years later his political career commenced, and he was appointed one of the Conservative Whips in the House of Lords. During Lord Salisbury's first two Administrations he was a Lord-in-Waiting, and in three successive years, 1887, 1888, and 1889, he was Lord High Commissioner to the General Assembly of the Church of Scotland. In this capacity he gained his first experience of representing his Sovereign, and he showed so much promise that in the latter year, when the Governorship of Victoria became vacant, he was appointed to fill the post. His success there was complete, and his popularity extended far beyond his own Colony. He returned home in 1895, and was immediately appointed a Privy Councillor and Paymaster-General in Lord Salisbury's third Administration. He also, at that time, represented the Admiralty in the House of Lords. In 1898 Lord Hopetoun was appointed Lord Chamberlain, and filled that office till 1900, when, as already mentioned, he was nominated first Governor-General of the Australian Commonwealth. His tenure of this office gave the greatest satisfaction to the Australians, but for reasons which need not be entered into here, Lord Hopetoun did not see his way to retain the post for the full period for which he was appointed. In May, 1902, he announced his intention to resign, and he finally quitted office in the following July.

On the way home he stopped in India, where he had the misfortune to contract typhoid fever, and this illness probably undermined a naturally weak constitution. For three years he retired from politics, and devoted himself to country pursuits. His health at one time improved greatly, and his friends hoped that he would eventually be restored to active public life. So, indeed, he was for a short time. When Mr. Graham Murray, in 1905, vacated the office of Secretary for Scotland to become Lord President of the Court of Session, Lord Linlithgow was offered and accepted the former post, thus becoming a member of Mr. Balfour's Administration.

Last summer, to the great regret of all his friends, Lord Linlithgow's health took a turn for the worse. It was decided that he should spend the winter at Pau, but he failed to improve, and died somewhat suddenly at the commencement of March last.

Before his departure as Governor-General of Australia, he was created a Knight of the Thistle and a Grand Cross of the Victorian Order. He had been made a Grand Cross of the Order of St. Michael and St. George when he first went out to Victoria. For his services in Australia Lord Hopetoun was created Marquess of Linlithgow.

Lord Linlithgow married, in 1886, the Hon. Hersey De Moleyns, daughter of the fourth Lord Ventry. Lady Linlithgow was a good friend to this Institution, and whenever it lay in her power, notably during the festivities connected with the International

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Congress of 1897, she did all she could to assist her husband in the discharge of his Presidential duties. Two sons and one daughter survive their father, whose death in the very prime of life has caused such widespread sympathy with those who are left to mourn his loss.

Of the late Lord Linlithgow's career enough has been said to indicate his remarkable success in many and varied posts of highest responsibility. This success was due in large measure to the great gifts and natural qualities with which he was endowed, and which he used to such advantage. Sincerity was the keynote of his character, and this quality, coupled with remarkable lucidity of intellect, with an unlimited capacity for taking pains, and with quite unusual moral and social gifts and personal charm, accounted for his extraordinary success in the discharge of the duties of the offices which he undertook. No man had a keener sense of justice, and his kindness of heart and consideration for others were proverbial amongst his numerous friends.

JOHN CORRY, J.P.

Mr. John Corry, J.P., whose death occurred on February 17 last, in his 77th year, was a son of Mr. Robert Corry, of Belfast. In his earlier years he devoted much time to scientific pursuits, for which he showed considerable natural aptitude, and particularly for the study of architecture, which he followed with much assiduity, without, however, adopting it as a profession. So successful was he in this line that he was practically the responsible designer of several important buildings, and was the builder of others, while in later years he took an active share in the design of the business premises of the firms with which he was associated.

The firm of James P. Corry & Co., of London and Belfast, with which the subject of this memoir was so long and honourably connected, was founded in the early fifties by his father and his eldest brother James (afterwards Sir James Corry, Bart.), who for many years represented Belfast in Parliament. Their first iron sailing ship, the *Jane Porter*, was built in 1859, but subsequent vessels took the "Star" names that still characterise that line. These early sailing ships were engaged in the East Indian trade, and made some fine runs to and from Calcutta, but eventually (though not until 1887) the firm decided to replace them by steamers, and to engage in the Australian, New Zealand and River Plate frozen meat trade, with which the Star Line has ever since been connected.

Mr. John Corry took a keen and practical interest in everything that affected the shipowner, and more particularly in the technical details of shipbuilding and marine engineering that necessarily came under his notice. The soundness of his technical knowledge, which he appeared to acquire with a natural facility, served him in good stead, and was frequently a source of surprise to the trained shipbuilders and engineers with whom he was brought into contact. It was this interest in technical matters that led Mr. Corry to take an active part in the meetings and work of the

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Institution of Naval Architects, which he joined as an Associate in 1874. He attended the meetings with much assiduity, and frequently took part in discussions, bringing out valuable information by his pertinent questions and intelligent criticism of matters viewed from the shipowner's standpoint. He was elected to a seat on the Council in 1886, and became one of the Trustees of the Institution in 1894. Only a year before his death he was, in recognition of his long and valuable services to the Institution, elected an Honorary Vice-President.

His connection with the General Shipowners' Society was of equally long standing, and in 1885 he occupied the responsible office of President of the Chamber of Shipping of the United Kingdom.

Mr. Corry was one of the oldest and most respected members of the Committee of Lloyd's Register of Shipping, having served on that body since 1876. He took an active interest in the affairs of the Society, which derived great advantage from his technical knowledge and wide practical experience.

His frank and genial manner, and the keen interest he took in a wide range of subjects, had won him a large circle of friends, whose sympathy will be heartily extended to his widow and family in their bereavement.

JOHN MACFARLANE GRAY.

The Institution has lost by death one of its oldest and most valued members in the person of Mr. John Macfarlane Gray, who passed away at Edinburgh in January last, aged 76 years. Mr. Macfarlane Gray became a member of the Institution in 1870, and was elected Member of Council in 1880 and Vice-President in 1904. He contributed many papers to the Transactions, and frequently took part in the discussions at the meetings, his contributions to debate being always valuable.

Mr. Gray was apprenticed at Edinburgh, and it was during his apprenticeship that he commenced the study of mathematics, mechanics, and science which he continued throughout his life. His first connection with marine engineering was at Messrs. McNab's works at Greenock. Subsequently he transferred his services to Messrs. Blackwood & Gordon, at Port Glasgow, and from this position he became the chief draughtsman and manager to Messrs. G. Forrester & Co.'s establishment at Liverpool. Here he designed marine engines and machinery of various types, and in 1866 he brought out his steam steering gear, which was first applied to the *Great Eastern*. Up to this time all ships had been steered by hand, but with the increase of size and speed of vessels, the power required for putting over the helm had become so great, and the time occupied so long, that mechanical power had become necessary. His invention of the differential gear rendered steam steering a possibility, and its principle is to be found in all the mechanical steering gears which have since been used.

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He left Messrs. Forrester's to join the Board of Trade. At first he was stationed at Liverpool, and afterwards at Cork. Subsequently he was transferred to London, where he was specially engaged in assisting in drawing up the first published set of rules dealing with the details of boiler construction. He was afterwards appointed Chief Examiner of Engineers, for which position he was admirably suited. This brought him in contact with a large number of our marine engineers, and laid the foundation of many lifelong friendships.

On the formation of the Institute of Marine Engineers he became one of its first Vice-Presidents, and he did much to help that institution to attain its present state of usefulness, and to the last he took a keen interest in its meetings and proceedings.

It was, perhaps, the combination of scientific attainments with sound practical knowledge which rendered his work so useful to the profession. His work in connection with the Ether Pressure Theory and with the Theta-Phi diagram was original and valuable, but much of it was never published. His first paper on the Ether Pressure Theory was read at the Institution of Naval Architects in 1880, but at his special request it was not published in the Transactions. In 1885 he contributed another paper to the Institution of Naval Architects, on the "Theoretical Duty of Heat in the Steam Engine," which contained results obtained on the lines of the ether pressure theory. In 1889 he contributed another paper to the Institution, on "The Ether Pressure Theory of Thermo-Dynamics applied to Steam." At the Paris meeting of the Institution of Mechanical Engineers in the same year he read a further paper on "The Rationalisation of Regnault's Experiments on Steam." In the next year he read a further paper on the theory at the Royal Society, and in 1901 he contributed a review of the whole subject to the Institution of Civil Engineers in a paper on "Variable and Absolute Specific Heats of Water." For this paper he was awarded a Telford medal.

His death will be regretted by a wide circle of personal friends.

HENRY FREDERICK SWAN, C.B.

Mr. Henry Frederick Swan was born on September 10, 1842, at West Farm, Walker-on-Tyne, and, after a period of private education, served his apprenticeship from 1858 to 1862 with Messrs. Charles Mitchell & Co., Iron Shipbuilders, of Walker-on-Tyne, the yard which is now the mercantile shipbuilding yard of Messrs. Sir W. G. Armstrong, Whitworth & Co.

On the completion of his apprenticeship he was sent to represent the firm, and to take charge of a dockyard at St. Petersburg, where the Russian Government intended to build five armour-clad coast-defence vessels, the *Netron Menya*, *Prince Pojarski*, *Rusalka*, *Czarodeika*, and *Smertz*.

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Iron shipbuilding was but little understood in Russia in those days, and it was Mr. Swan's pioneer work that laid the foundation of the considerable shipbuilding yards which are to be found on the Neva. His work here was carried out with complete satisfaction to the Russian Government. The Emperor marked his approval by a present of a gold snuff box on the completion of his mission, and on his return to England he was appointed to take over the management of the yard at Walker, the senior partner, Mr. Mitchell, being at that time obliged to give up a large part of his work owing to ill-health.

The work carried out by the Walker Yard, under Mr. Swan's management, was at first largely for Russia, for which country a number of vessels, principally of small size, were built. Among them were several paddle steamers of considerable speed, besides cargo and passenger steamers. Two large cable steamers, the *Faraday* and the *Hooper*, were produced about this time, the latter being remarkable for the short time (100 working days) in which she was built, an achievement which, with the machinery and appliances then in use, speaks eloquently for the management.

The shipyard was from time to time improved and enlarged, and the output, which was 9,542 tons in 1864, grew to 26,030 tons in 1882. In the latter year an amalgamation was made with Sir W. G. Armstrong & Co., and the style of the firm became Sir W. G. Armstrong, Mitchell & Co., Mr. Mitchell and Mr. Swan being made directors of the new company.

At that time it was intended to build warships at Walker, but this was found inadvisable, and a second shipyard was established at Elswick, exclusively devoted to the production of war vessels. The class of vessel with which Mr. Swan was peculiarly identified was the tank steamer. He designed in 1885 for the Atlantic trade the steamer *Glückauf*, which was practically the first oil-tank steamer specially constructed for this service, and he contributed some particulars of this vessel to the Transactions I.N.A. He interested himself very largely in all questions relating to petroleum, its transport and use as fuel, and to his efforts is due the fact that over 90 tank steamers have been built at Walker. The icebreaking steamer also received considerable attention from Mr. Swan, and in 1880 he contributed a valuable paper on this subject to the Summer Meeting of the Institution at Newcastle-on-Tyne, when the late Admiral Makaroff, who took the deepest interest in the construction of these vessels, was present, and took part in the discussion. Such examples as the *Volga Icebreaker*, the *Volga Ferry*, the *Baikal*, and the *Ermack*, besides a number of smaller steamers of this class, show the extent to which the subject was developed. Passenger and cargo steamers have been produced in large numbers from the Walker Shipyard, and during the period when Mr. Swan (or Colonel Swan, as he was more generally known) was the active head of the Department, the output rose from 9,542 in 1864 to about 37,000 to 40,000 tons at the present day. He was also connected with the Wallsend Slipway and Engineering Co., acting as Chairman of that Company for some years; and was a director of the Cargo Fleet Iron Co. and the Weardale Steel Co.

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Like many other distinguished leaders in the shipbuilding world, Colonel Swan had other interests besides his professional ones. He was an ardent Volunteer, and for over 40 years took active part in the fortunes of the 2nd V.B. of the Northumberland Fusiliers, in which he held an active command for upwards of 25 years. On his retirement in 1902 he was presented by his brother officers with a sword of honour and created a C.B. and an Honorary Colonel of his regiment.

In the affairs of the Institution of Naval Architects (which he joined in 1871) he always took the deepest interest, and was prominent among those who hospitably received the members at Newcastle in 1899. He served upon the Council from 1881 to 1884 and from 1902 until his death. Of the North-East Coast Institution of Engineers and Shipbuilders, Colonel Swan was a Past-President, and he also belonged to the Institution of Civil Engineers, the Iron and Steel Institute and other technical Societies, while in the local administration of his own county of Northumberland he played an active part, and was serving as High Sheriff of that county at the time of his decease.

His death occurred very suddenly from heart failure at his residence, Prudhoe Hall, on March 25, and his loss will be deeply felt by the wide circle of friends which he has left. He leaves a widow, a daughter of the late W. B. Dawes, of the Hall, Kenilworth, and four sons and two daughters.

ROBERT THOMPSON, J.P.

The late Mr. Thompson, who died on January 1 this year, was the principal partner and Chairman of Directors of the old-established and well-known firm of Joseph L. Thompson & Sons, Ltd., of the North Sands Shipbuilding Yard, Sunderland. He was born on May 22, 1850, and was the eldest son of the late Joseph L. Thompson, and was educated at Gainford School.

Having been actively engaged in the shipbuilding industry since the year 1865, his experience was very varied, and there was probably no more popular business man in the Wearside Borough. From 1875 up to the origin of his illness, the fortunes of the shipbuilding business have been under the control and direction of Mr. Thompson, and the firm have, on a great many occasions, held the record of having launched the largest amount of tonnage on the Wear, and have held high positions in the world's annual output of tonnage, being fourth on three successive occasions, and a great deal of the credit for this is due to the untiring energy and business ability of Mr. Thompson.

Besides directing the affairs of the shipbuilding and repairing business, Mr. Thompson was an active partner in the Sunderland Forge and Engineering Company, Ltd.; he was one of the founders of the Wearmouth Foundry Company, Ltd.; Chairman of Directors of the Skinningrove Iron Company, Limited, Yorkshire; and was connected with various other business enterprises. He was a County Justice of

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the Peace, a Freeman of the Worshipful Company of Shipwrights, a Governor and Member of Council of the Durham College of Science, Newcastle-on-Tyne, and was one of the founders of the North-East Coast Institution of Engineers and Shipbuilders, of which he proved himself a popular occupant of the Presidential Chair during the two Sessions 1891-2 and 1892-3, and he contributed several valuable papers to the Transactions.

Mr. Thompson joined the Institution of Naval Architects in 1878, and was elected upon the Council in 1902. He was also a Member of the Council of the Iron and Steel Institute, and other learned Societies, and for a number of years was one of the representatives of the North-East Coast on the Committee of Lloyd's Register of British and Foreign Shipping, and acted as a member of the Technical Sub-Committee. He was also for a number of years a member of the River Wear Commission and of the Borough Council, and was connected with various charities in the Borough, being a member of the Infirmary Committee, and for a number of years Chairman of the Monkwearmouth and Southwick Hospital.

JOHN WIGHAM RICHARDSON, J.P.

Mr. John Wigham Richardson, who was born in 1837, came of Northern stock, being a member of the old Quaker family, known as the Richardsons of Cleveland (Yorkshire). His parents lived at Newcastle-on-Tyne, and here it was that the subject of this memoir received his first school training, subsequently going to the well-known Friends' School at York. His introduction to professional life was made in the shipyard of the late Jonathan Robson at Gateshead, and subsequently in that of R. & W. Hawthorn, which he entered at the age of 21. Previously to this, however, he had attended lectures at University College, London, and had travelled and studied abroad, thus laying the foundation of the wider interests which characterised and embellished his career. Mr. Richardson gave early evidence of an independent and resourceful spirit, for he soon severed his connection with the Hawthorn firm, and at the age of 23 he purchased an old-established shipyard on the Tyne—the Neptune Works—which had been founded by Mr. Coutts and had, eighteen years earlier, launched the first iron ship—the *Prince Albert*—built on the Tyne. The works had been subsequently carried on by Messrs. Miller, Ravenhill & Co., of London, but when taken over by Mr. Richardson they only covered four acres of land, and had a maximum output of 4,000 tons per annum from three small building berths. The successful development of this yard and engine works, under its able and progressive management, forms an interesting chapter in the history of the Tyne as a shipbuilding centre. In 1899 the firm was incorporated under the style of Wigham-Richardson & Co., Ltd., and four years later was amalgamated with Messrs. C. S. Swan & Hunter, Ltd., and the Tyne Pontoons and Dry Docks Co., Ltd., which latter company Mr. Richardson helped to found. The new company also acquired a

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large interest in the Wallsend Slipway and Engineering Company. This powerful combination thus secured control of shipyards and docks covering in all nearly 80 acres and with an output capacity of 150,000 tons from 16 building berths, the longest of which (900 ft.) had the distinction of seeing the *Mauretania* glide from its ways on Sept. 20, 1906. But it was with the Neptune Works that Mr. Richardson's professional career was for more than forty years closely identified, and it was in the direction of this shipyard and engine works that he displayed the splendid business ability and technical skill that enabled him to control successfully the still larger interests that awaited him.

His views on the revision of the rules laid down by the Classification Societies for the strength of ships, were strongly advocated in papers read by him before the Institution of Naval Architects (which he joined in 1875) and elsewhere, and he thus contributed to improvements that have since been carried out in the methods of determining the necessary scantlings. He was also an ardent advocate of the principle of co-operation of labour and capital in business, and he endeavoured, when his firm was converted into a limited liability concern, to give the workmen every opportunity of becoming shareholders in the company.

As has been already mentioned, his interests in life were not by any means confined to his business enterprises. He found time for travel at various periods of his career, undertaking journeys to such interesting spots as the battle-fields of the Crimea, Greece, Turkey, and the Franco-Prussian war, to the United States of America, South Africa, and the West Indies. A sincere lover of art, he freely supported local talent, and his home surroundings bore evidence of his good taste and discrimination. His cultured mind turned readily from the cares of business to the relaxation of social and domestic duties, and those who had the privilege of his friendship will recall his personal charm, and the varied gifts that combined to form a character of exceptional worth and strength.

He died on April 15, 1908, leaving a widow, daughter of the late Mr. J. P. Thoel, of Wycombe Marsh, Bucks, and three sons and two daughters to mourn his loss.

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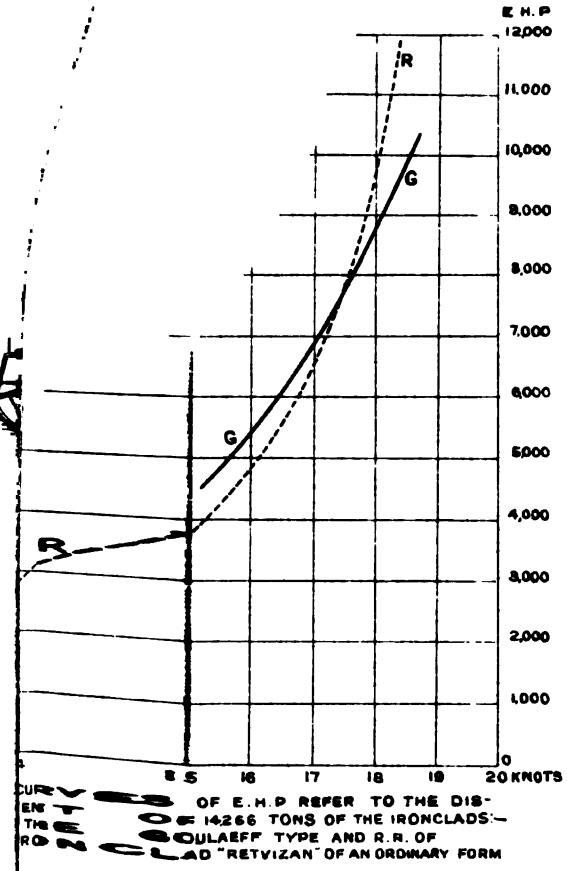

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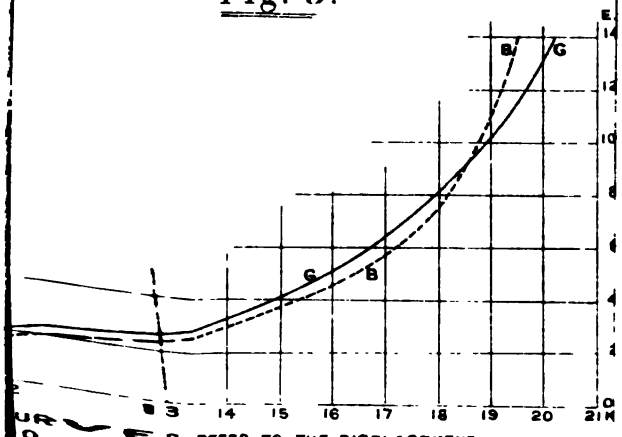

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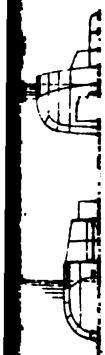

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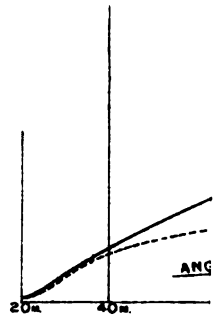

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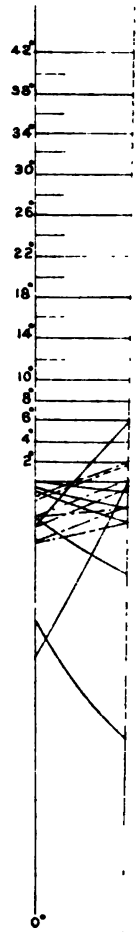

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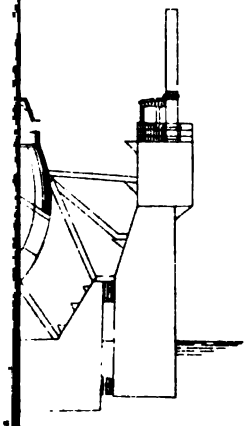

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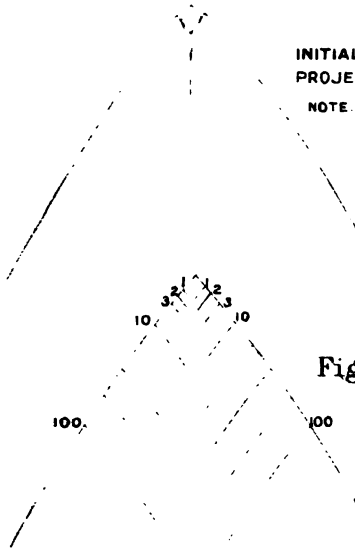

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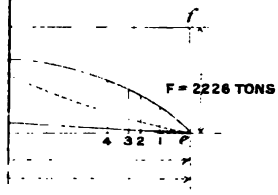

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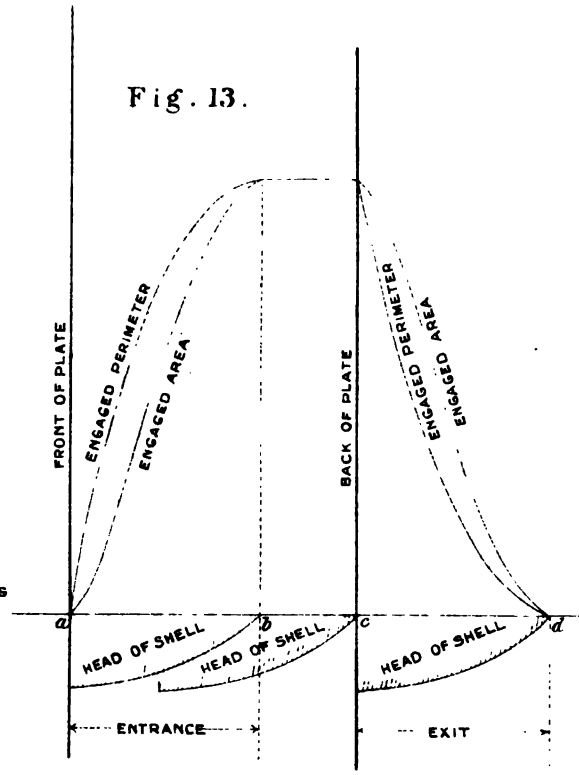

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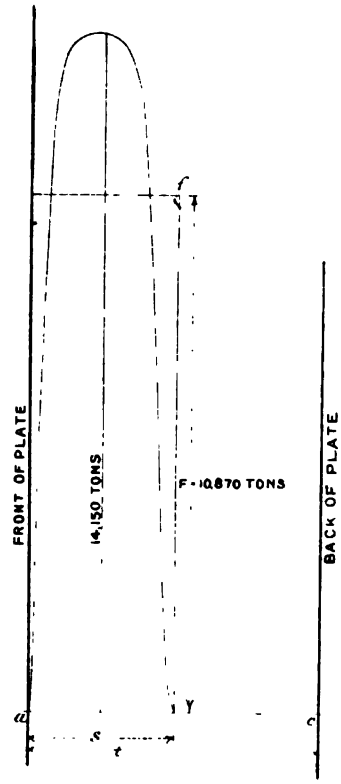
Fig. 15.

6" UNCAPPED SHELL PULVERISED ON IMPACT WITH 12" HARD FACED PLATE AT 2,800 F.S. PENETRATION 6"



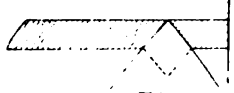
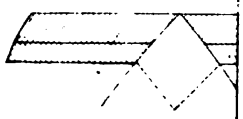


Fig. 4.

Fig. 14.

6" UNBREAKABLE UNCAPPED SHELL EXACTLY PERFORATING 12" HARD FACED PLATE AT 2,800 F.S. APPROXIMATE TOTAL STRESS DIAGRAM IN FULL LINE. MODIFICATION INTRODUCED BY CAP SHOWN DOTTED.



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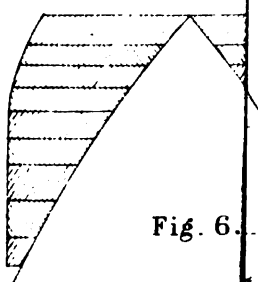

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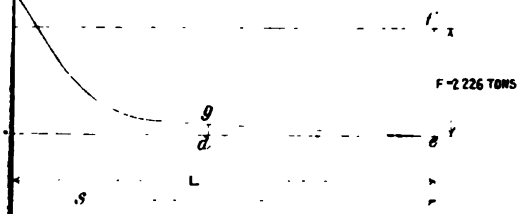

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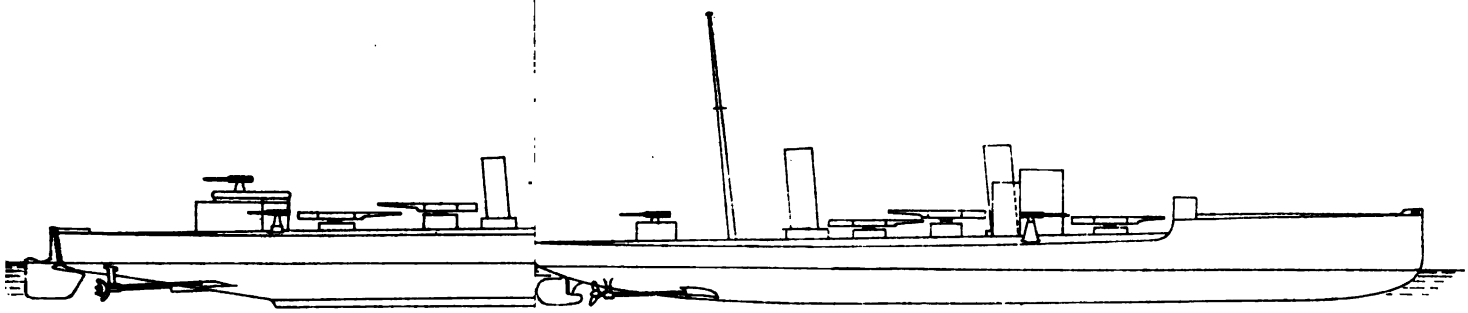


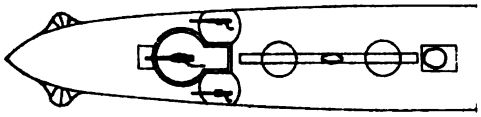
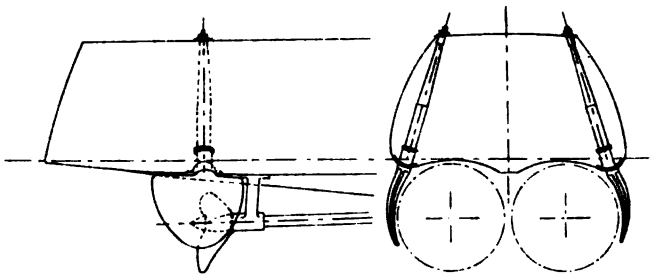
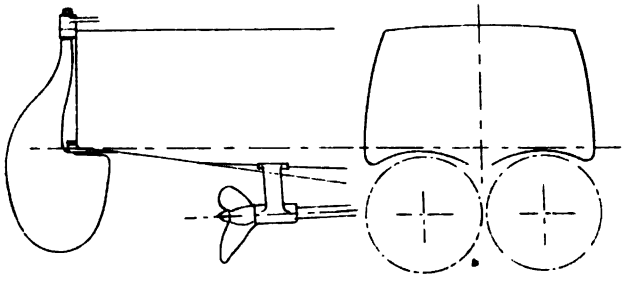


Fig. 6.

STERNS OF "THORNYCROFT" DESTROYERS WITH DOUBLE & SINGLE RUDDERS.





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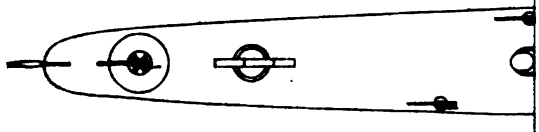

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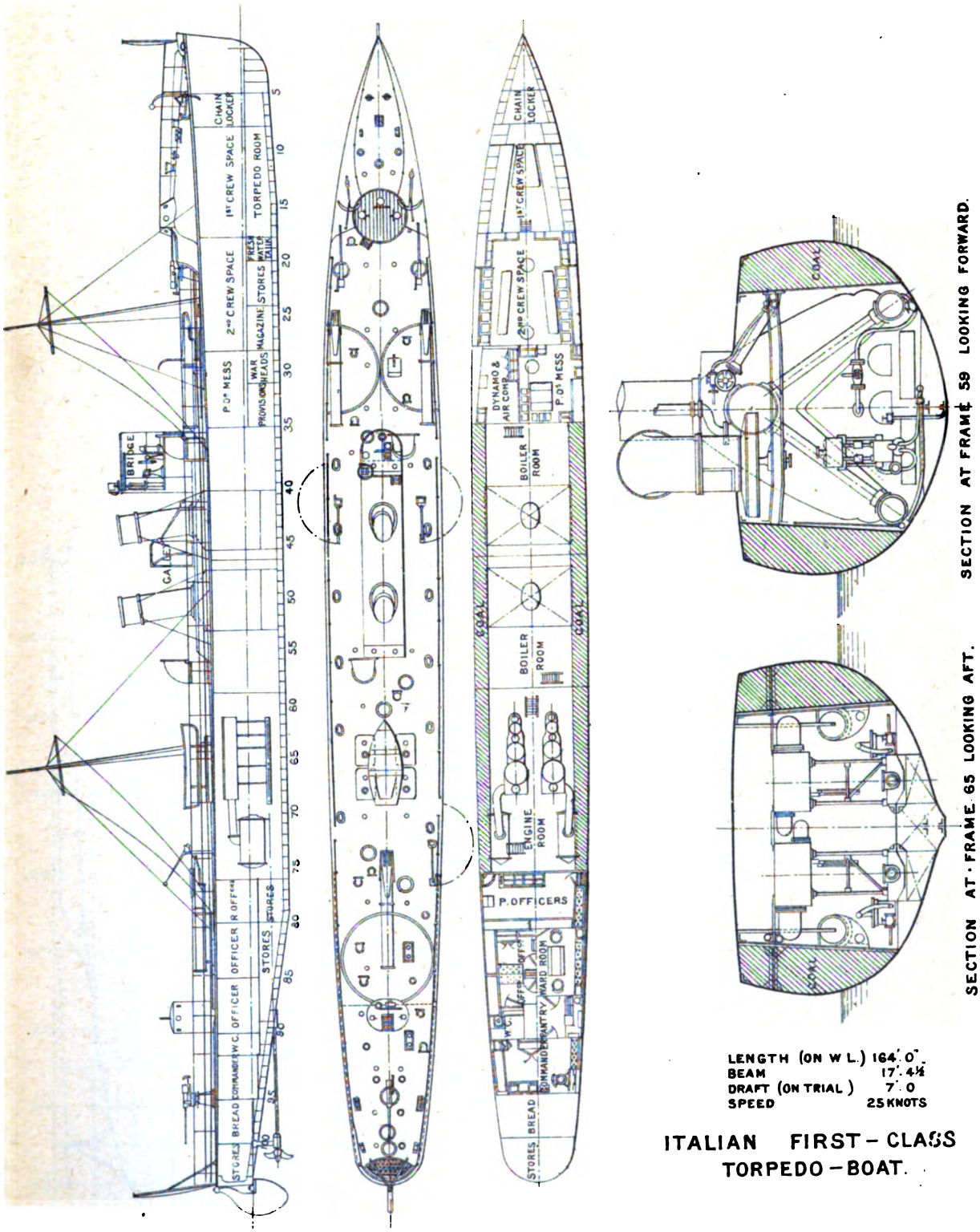

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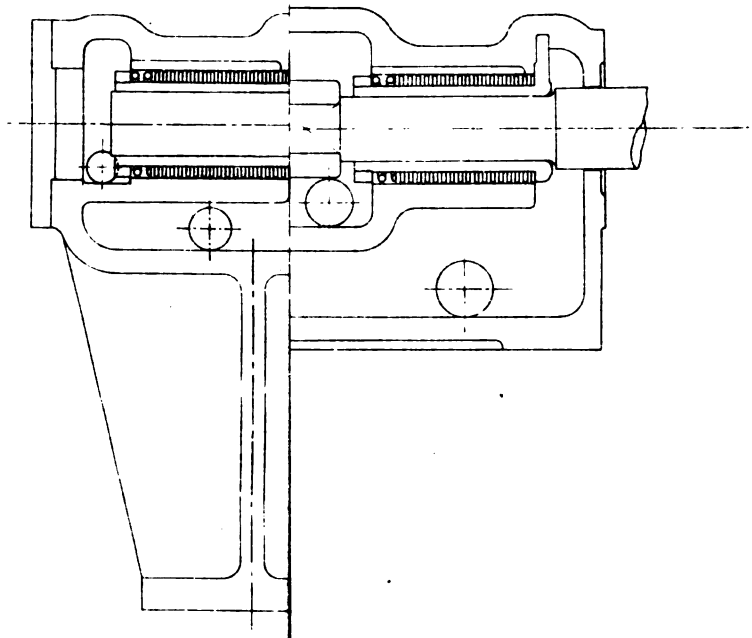

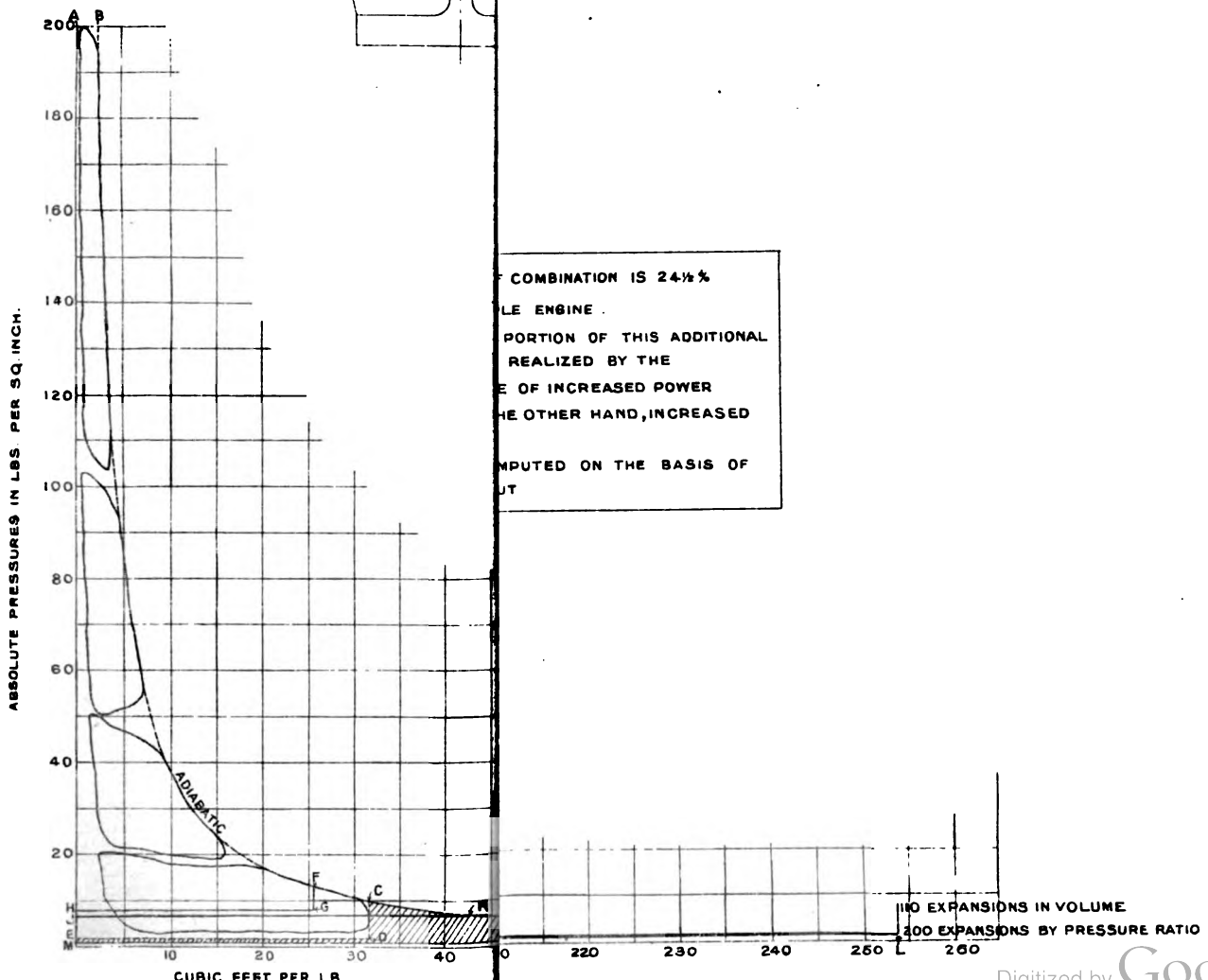

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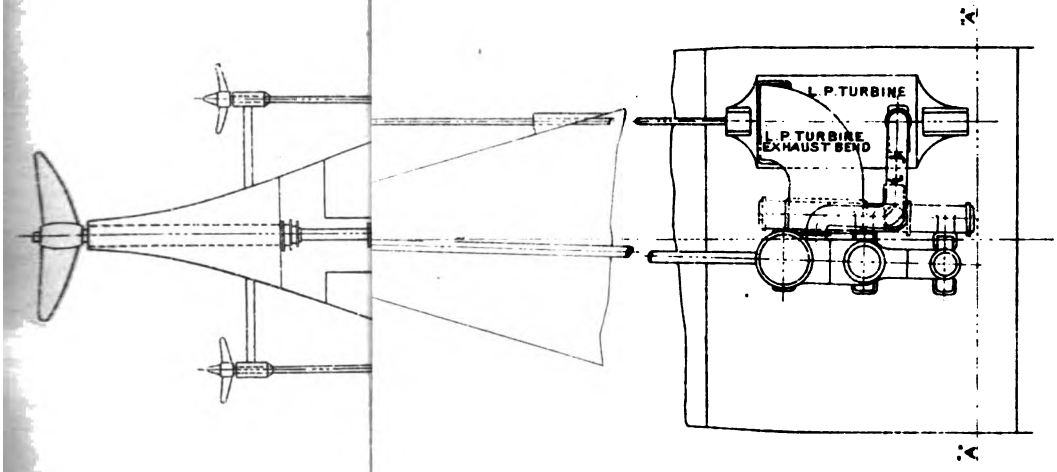
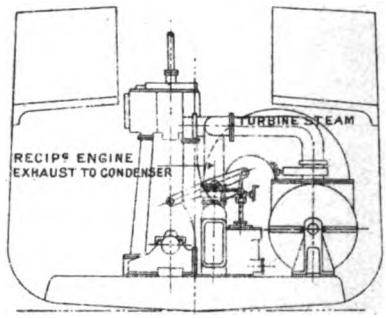
GENERAL ARRANGEMENT OF COMBINATION OF A TRIPLE EXPANSION RECIPROCATING ENGINE WITH ONE LOW PRESSURE TURBINE.





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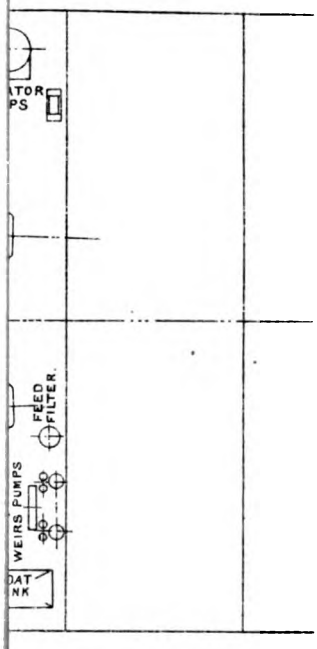

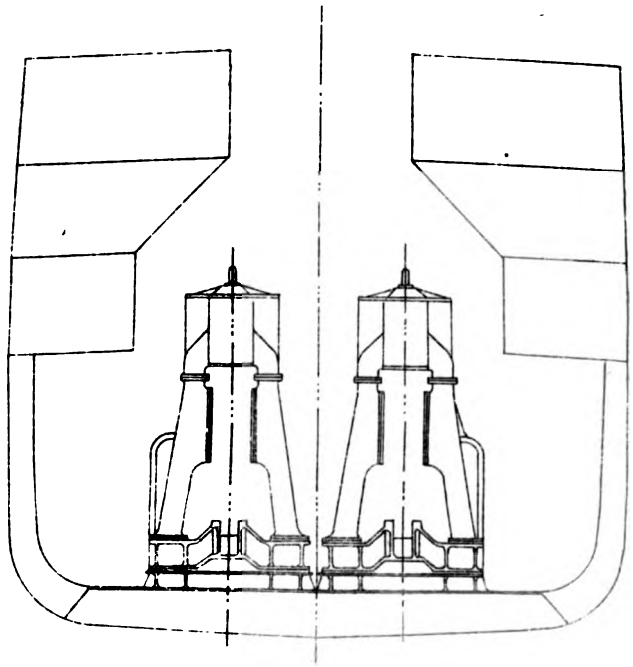

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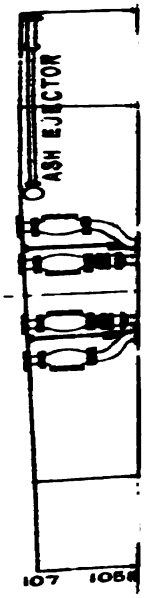

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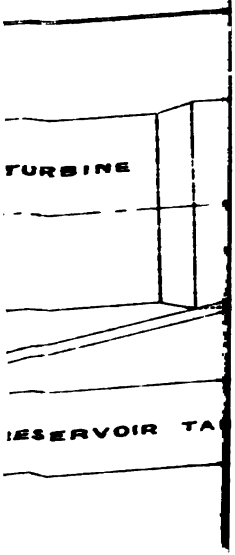

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Fig. 4.

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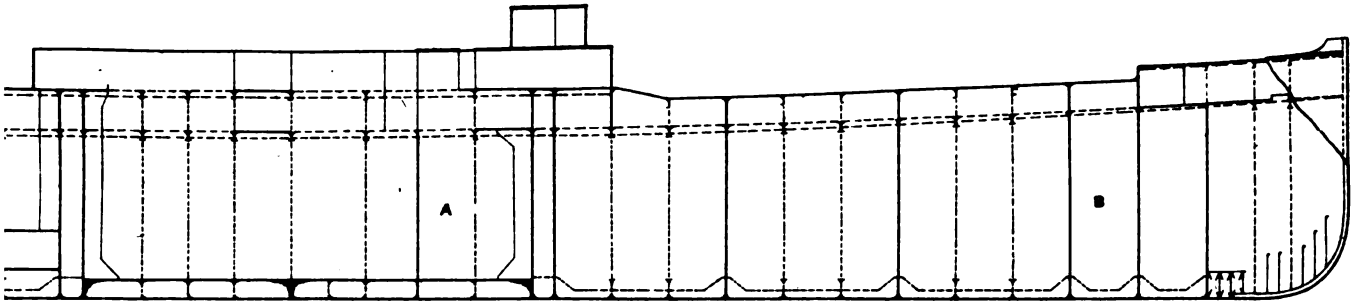

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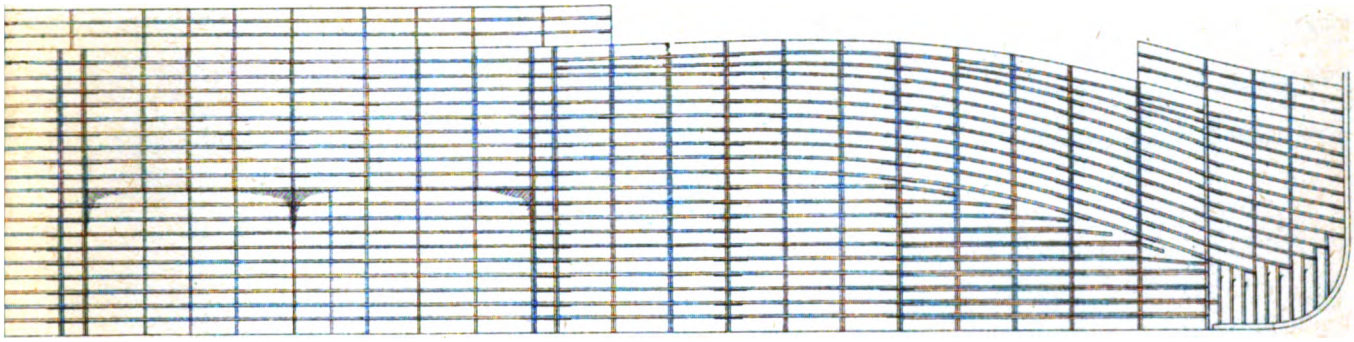

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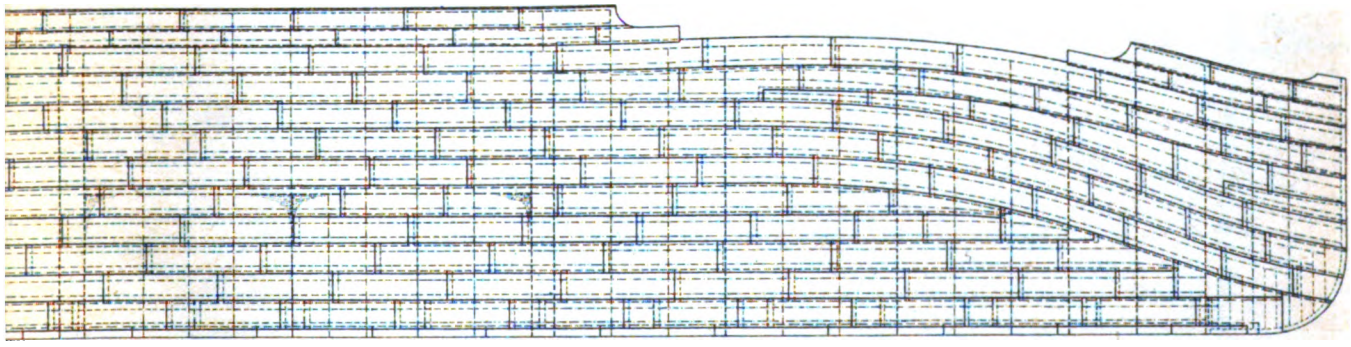

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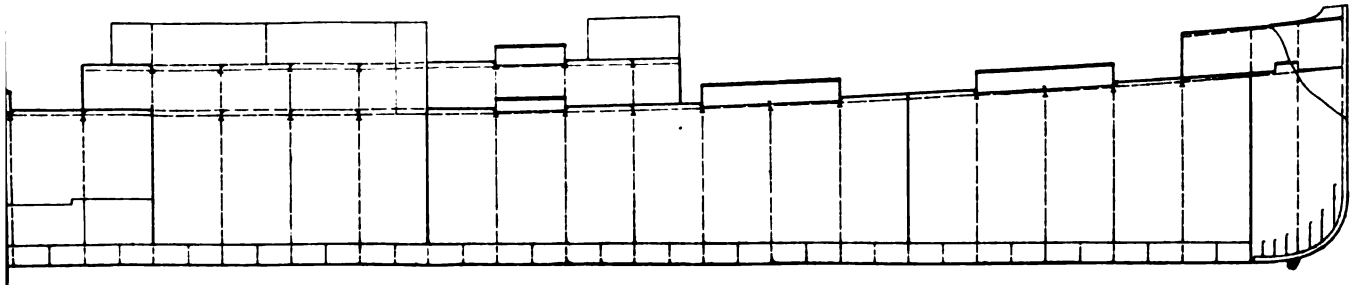

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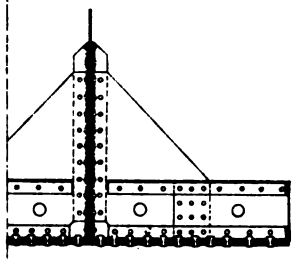

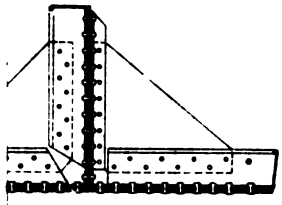

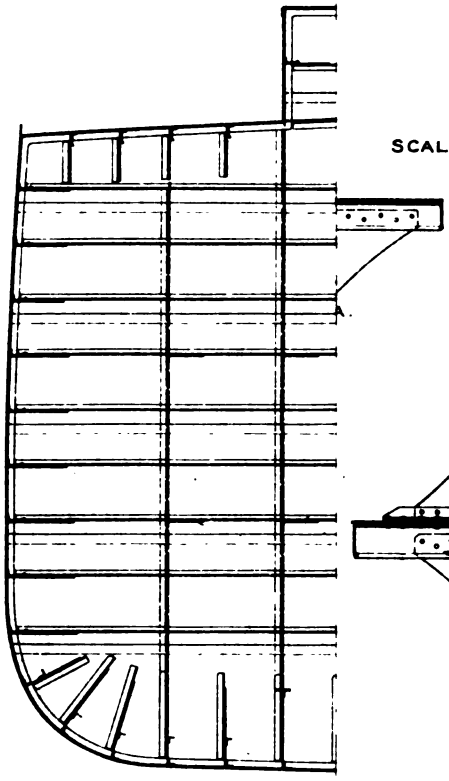

Fig. 9.

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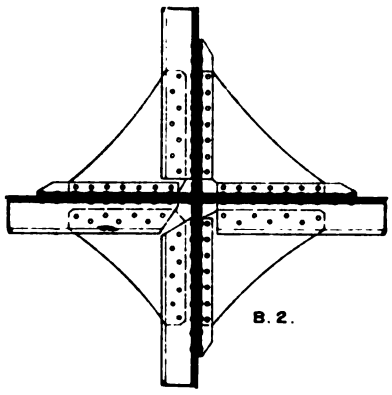

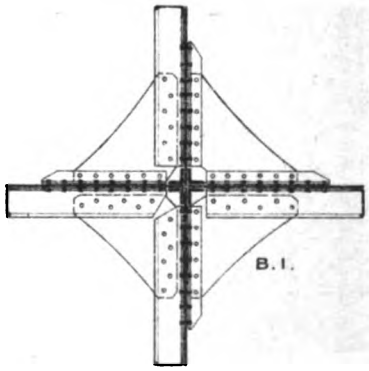

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TEMPERATURE

AVERAGE WATTS PER HOUR

TEMPERATURE

AVERAGE WATTS PER HOUR

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To Illustrate Mr. W. Carlile Wallace's Paper on "The Heating of Modern Ocean Liners."

Fig. 5.

VOYAGE WEST S.S."OCEANIC".

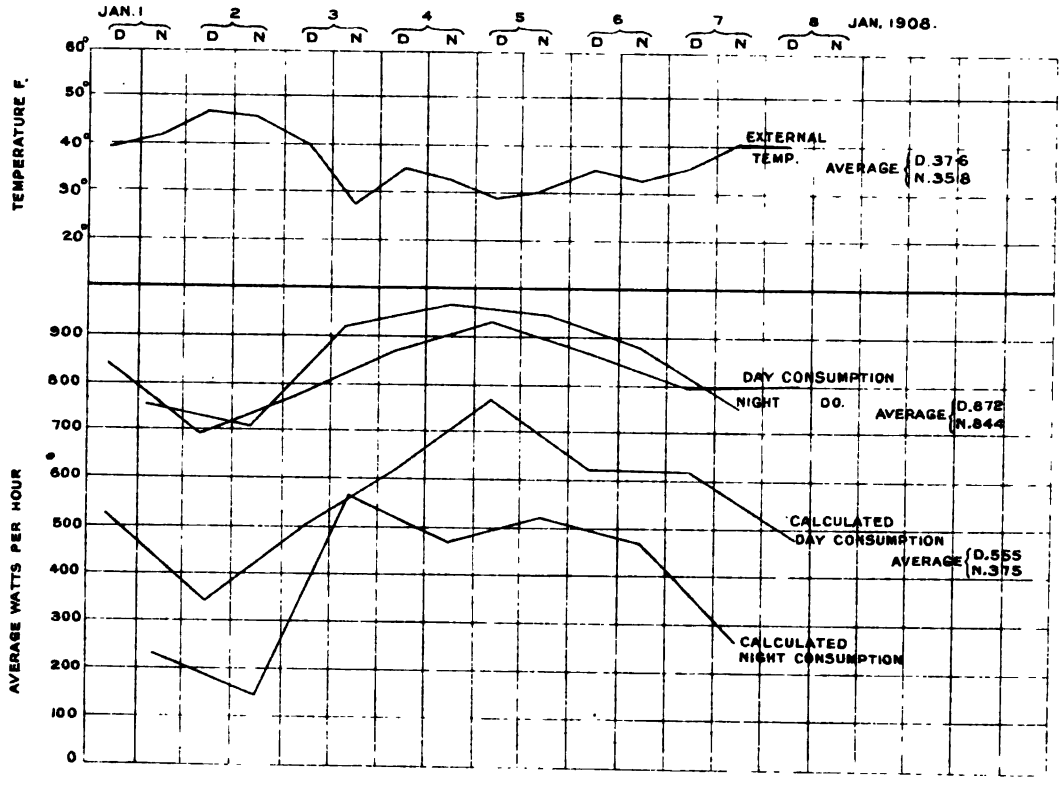
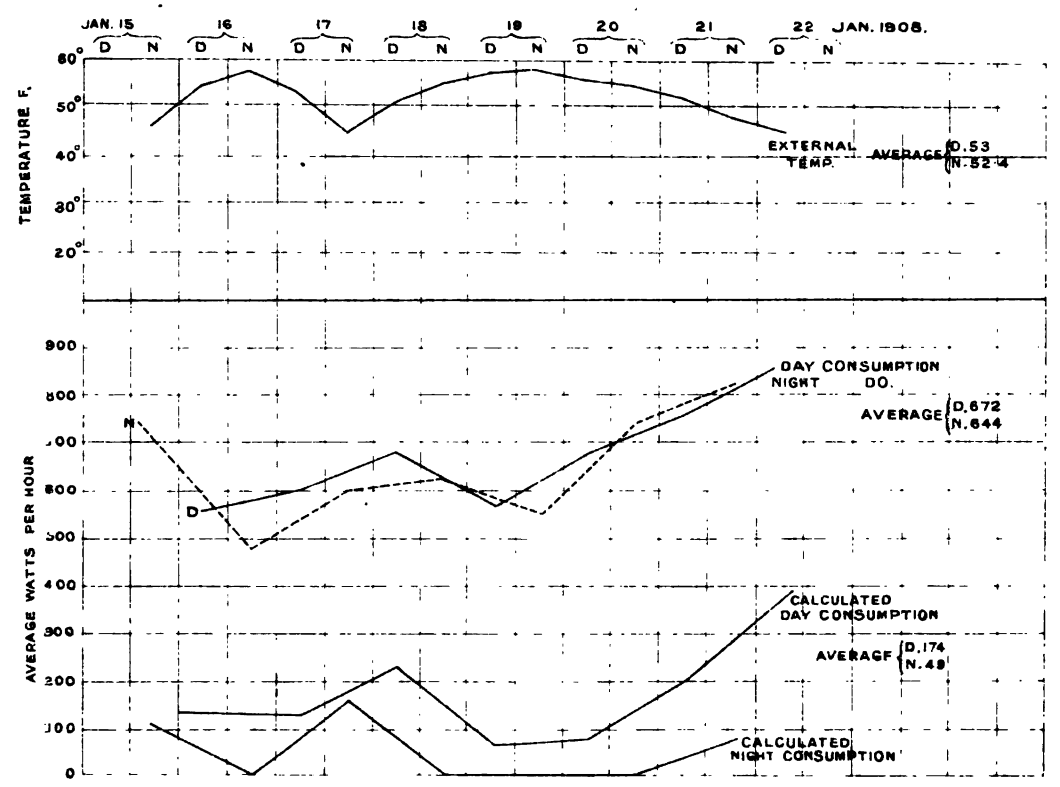


Fig. 6.

VOYAGE EAST S.S."OCEANIC".



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To Illustrate Mr. D. B. Morison's Paper on "The Influence of Air on Vacuum in Surface Condensers."

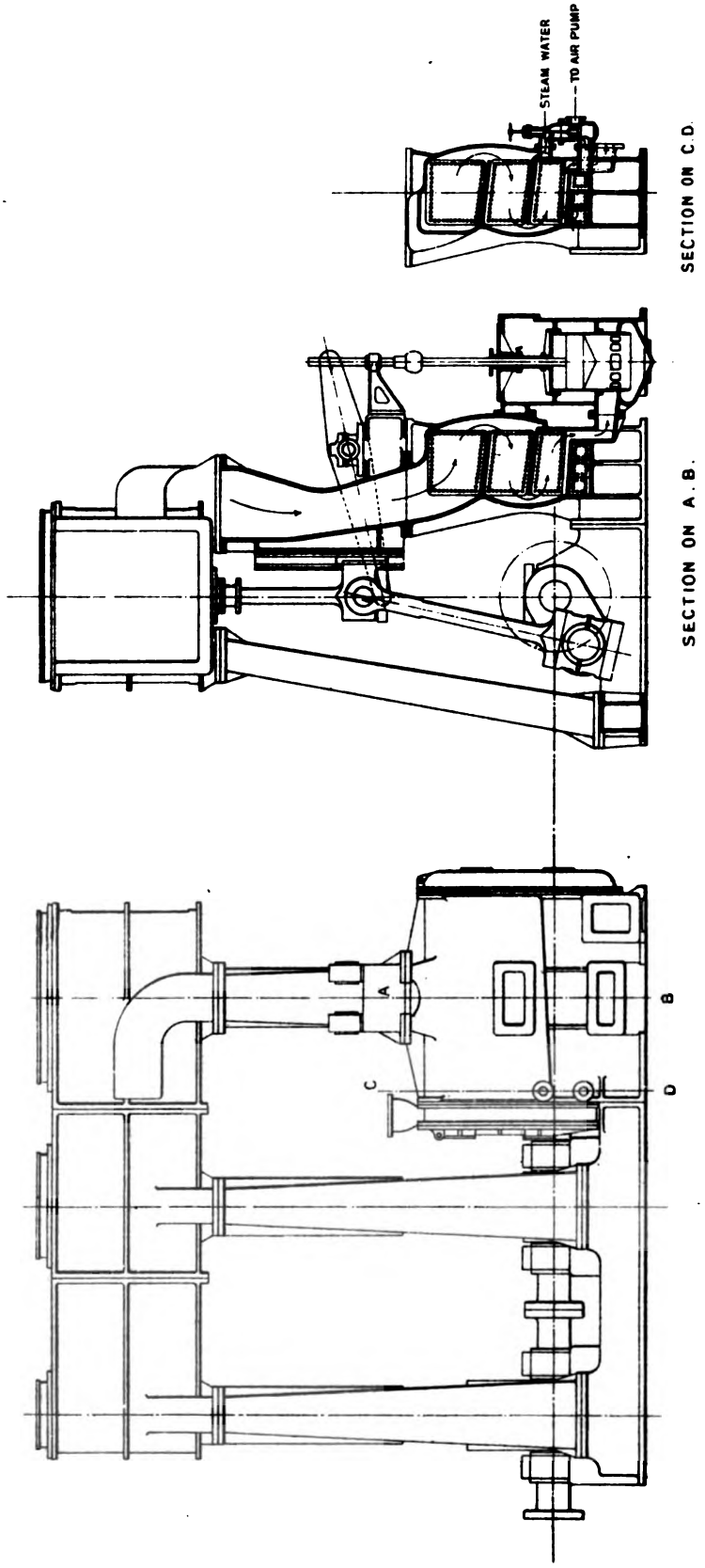

Fig. 1.

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To Illustrate Mr. D. B. Morison's Paper on "The Influence of Air on Vacuum in Surface Condensers."

Fig. 2.

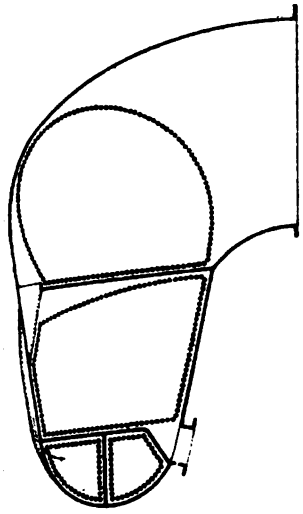


Fig. 3.

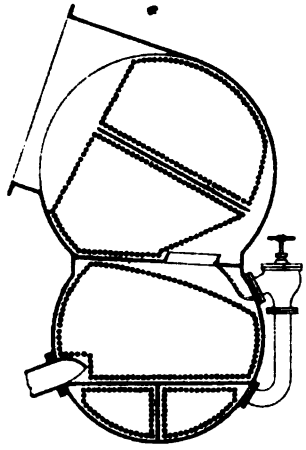


Fig. 4.

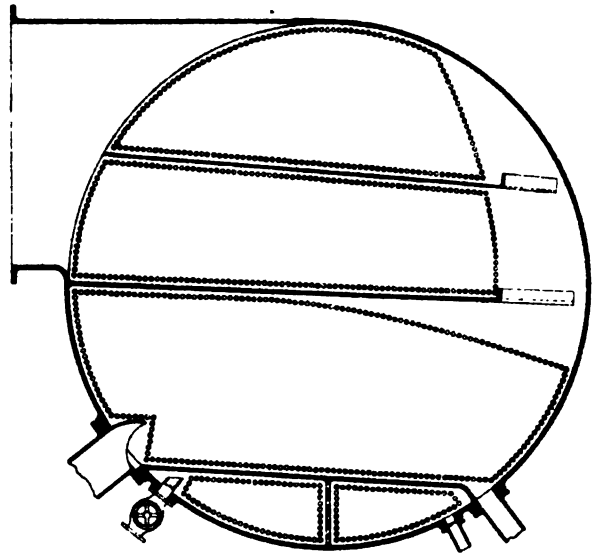


Fig. 5.

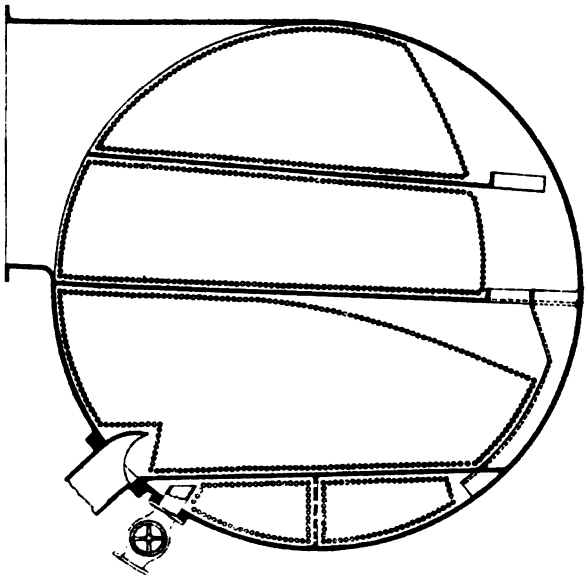


Fig. 6.

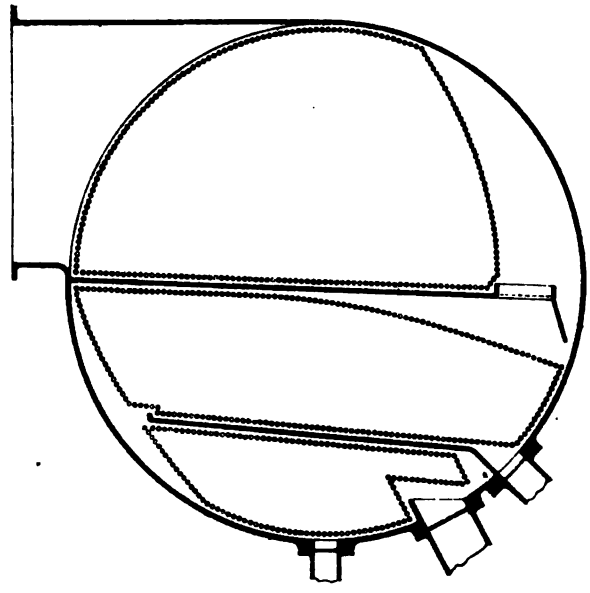


Fig. 7.

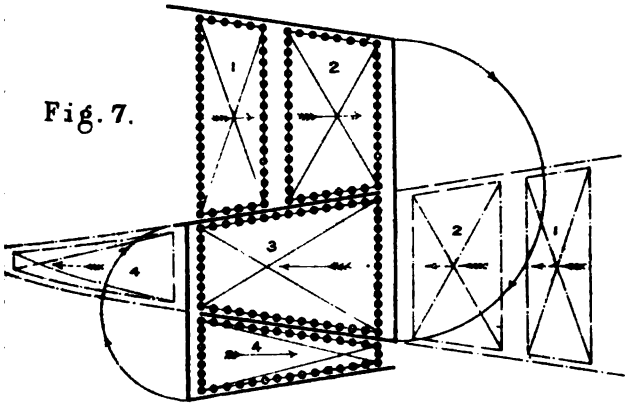
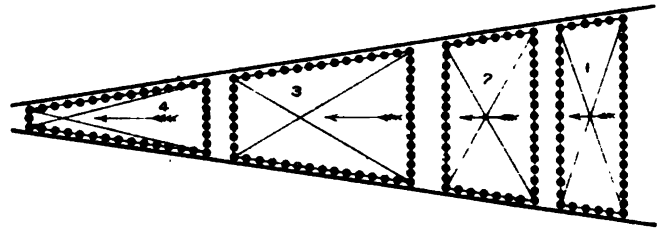


Fig. 8.



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To Illustrate Mr. D. B. Morison's Paper on "The Influence of Air on Vacuum in Surface Condensers."

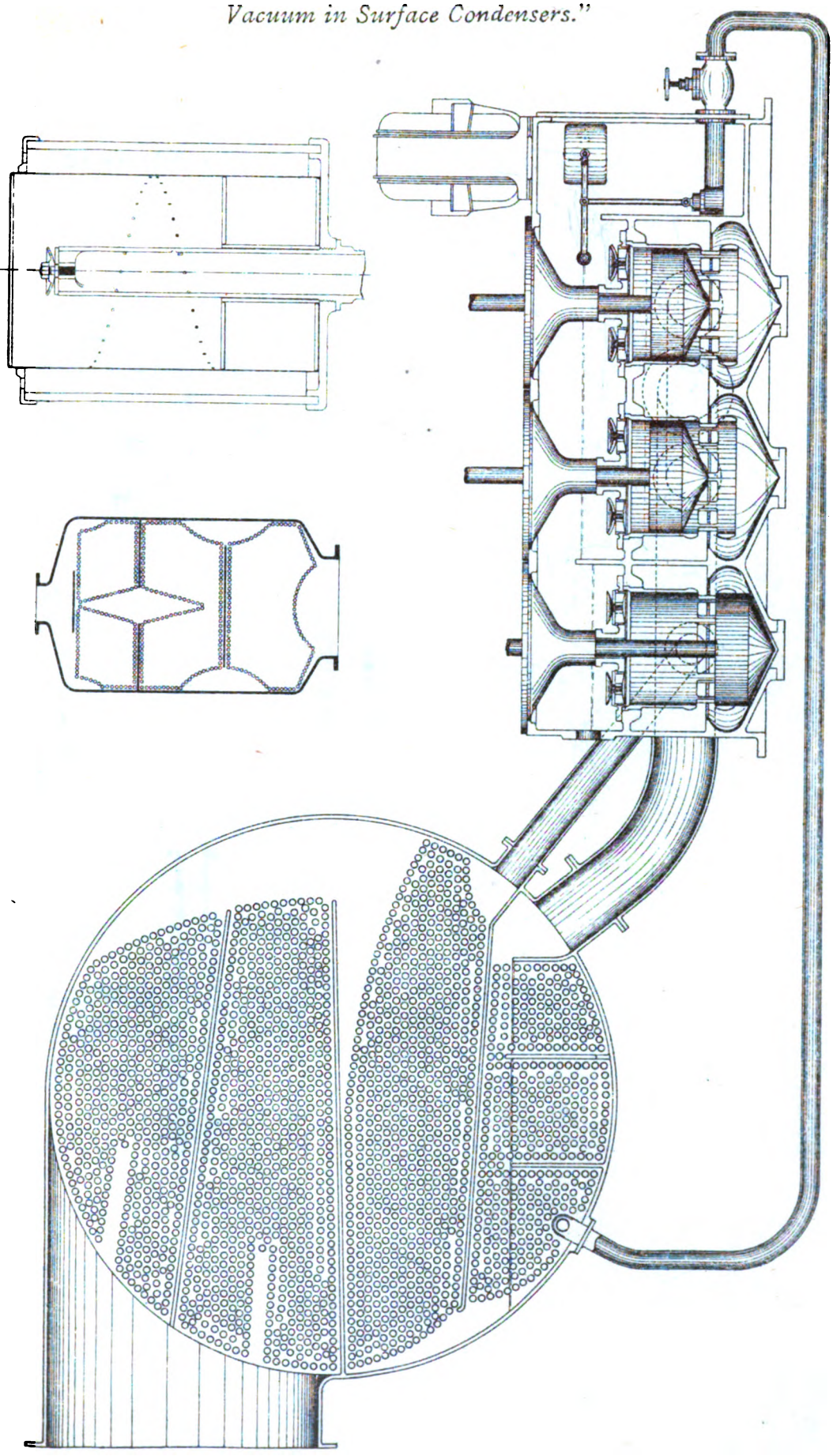

Fig 11.

Fig. 9.

Fig. 10.

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VALVE GEAR OF LOW PU" FI

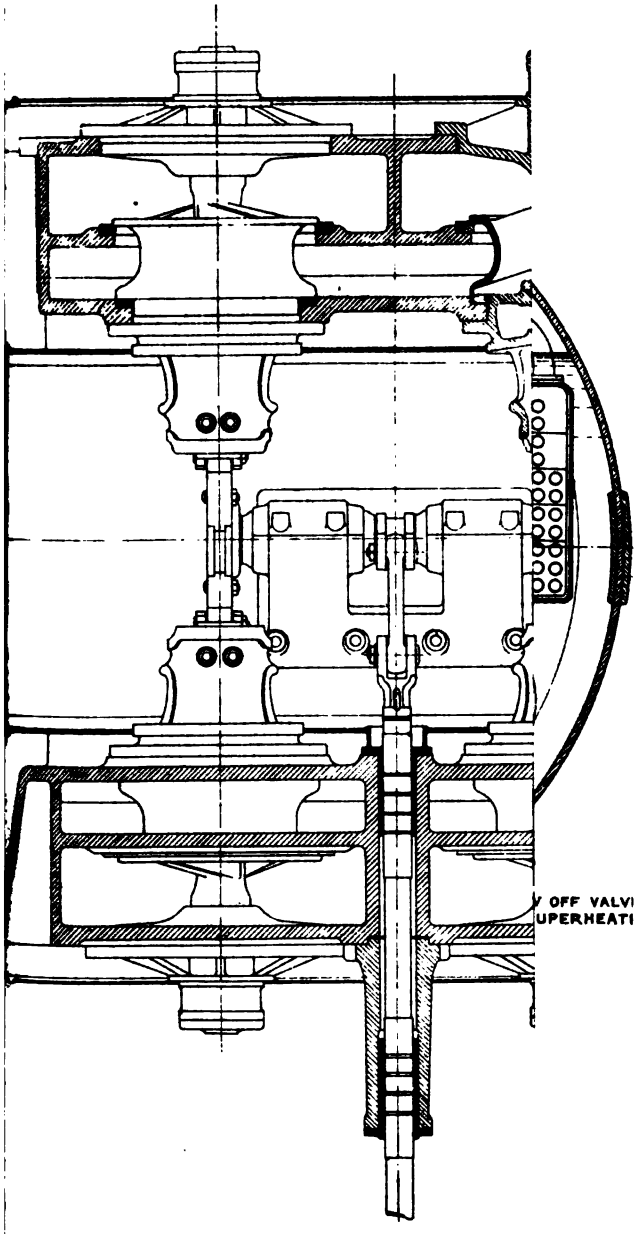

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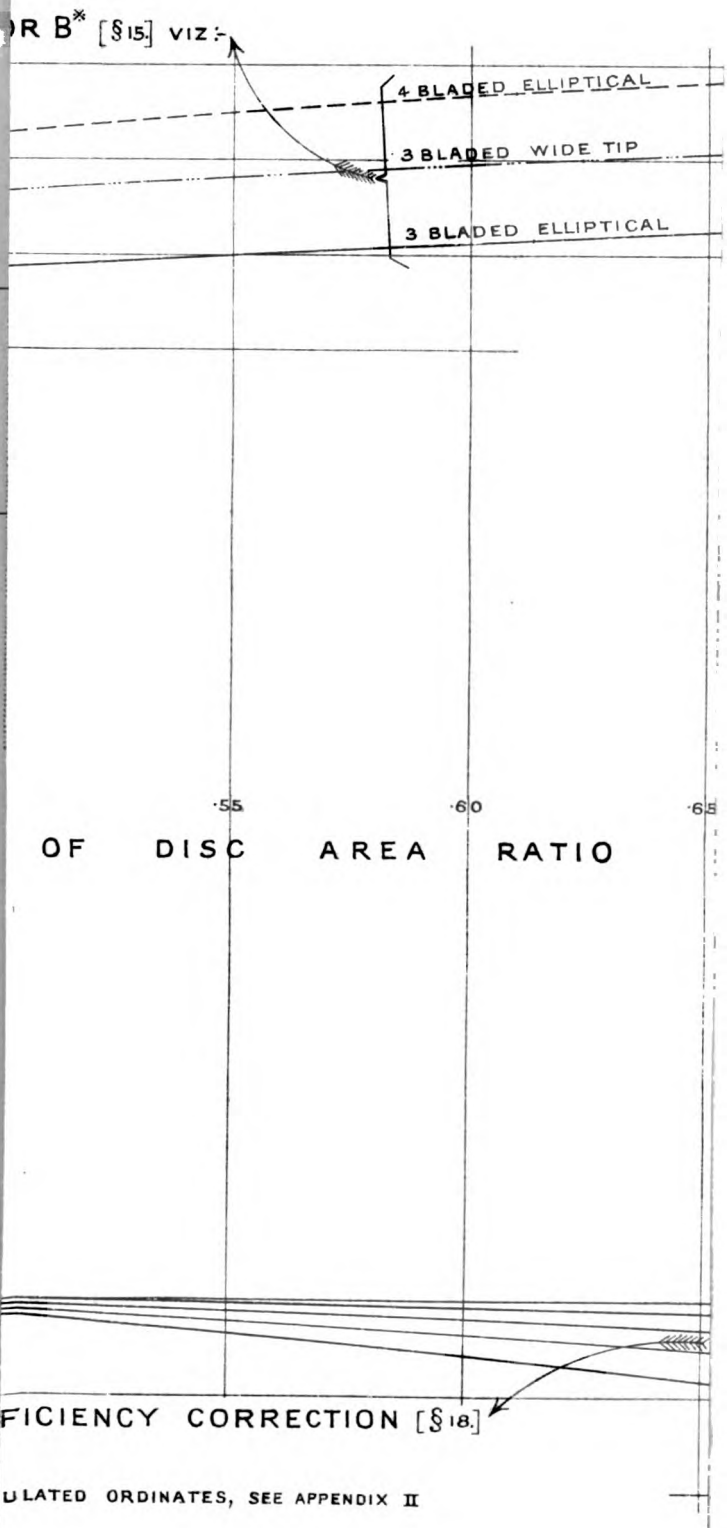
Fig. 5.



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Experiments

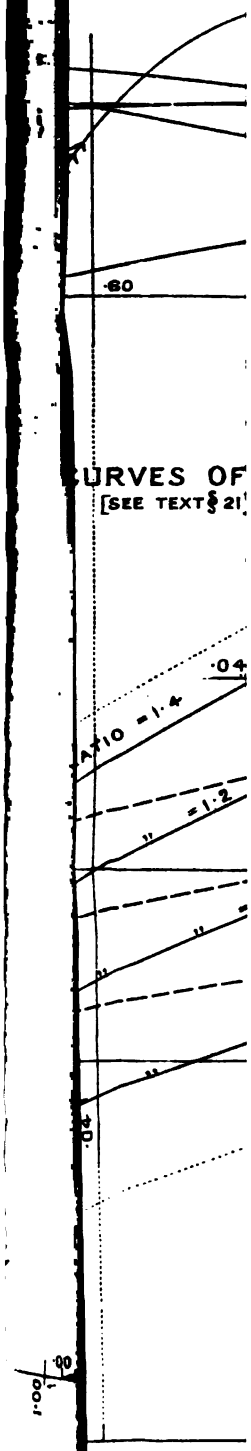

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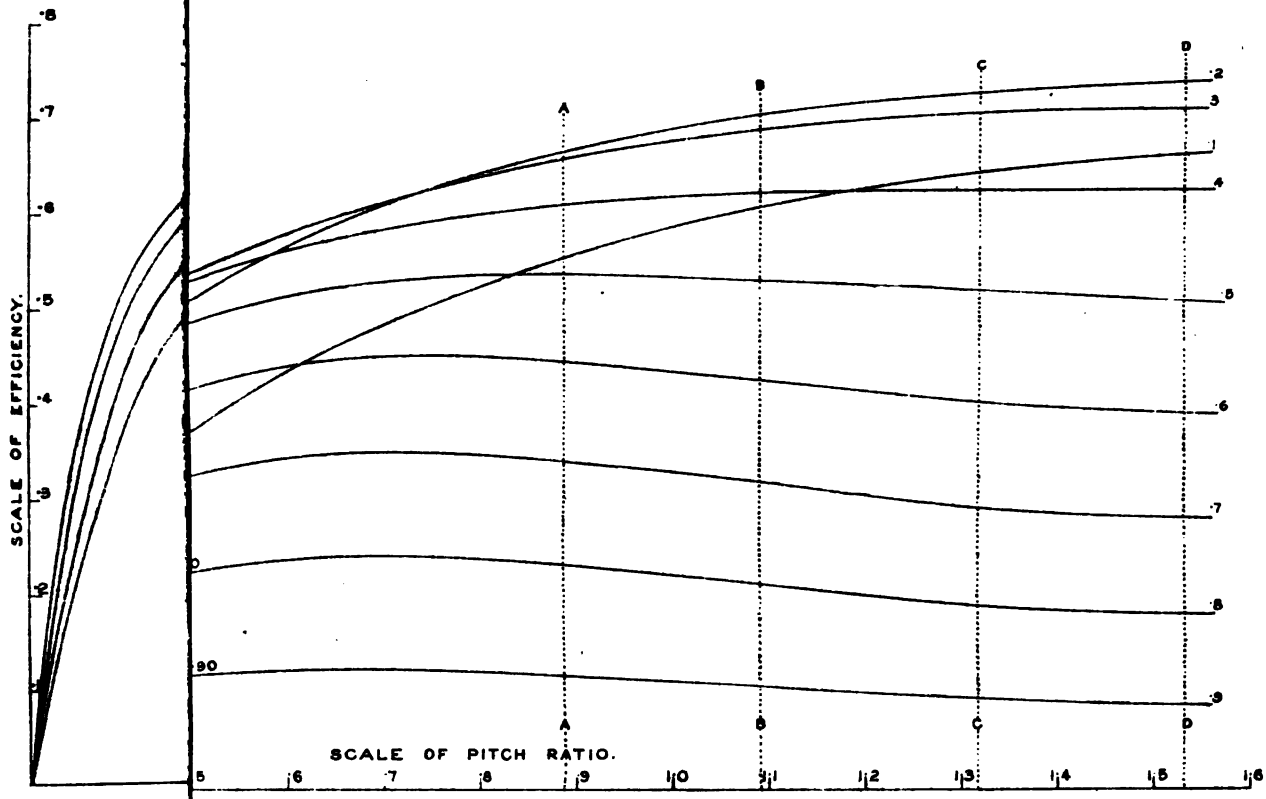
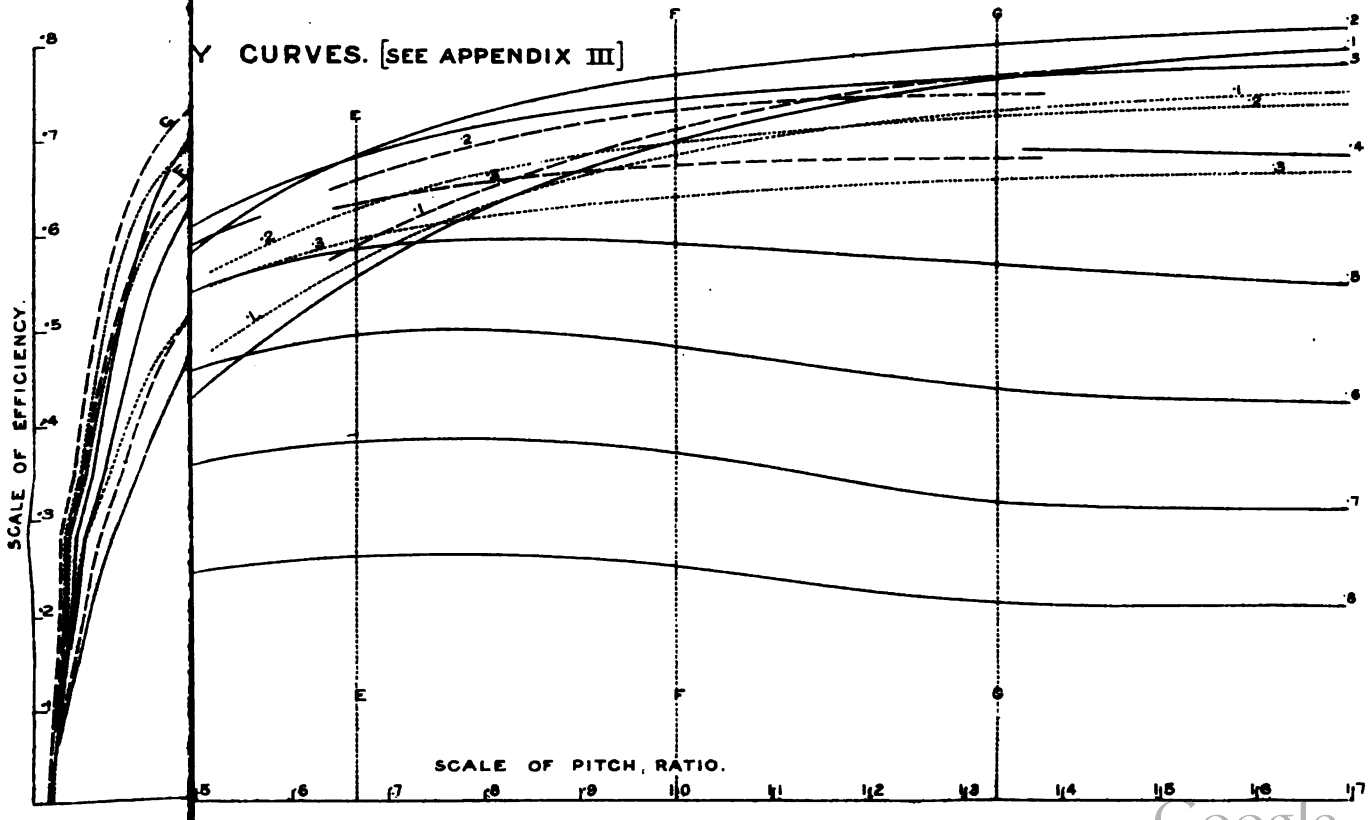
Fig. 9.

EXPERIMENT EFFICIENCY CURVES. [§ 20]



11.

Y CURVES. [SEE APPENDIX III]



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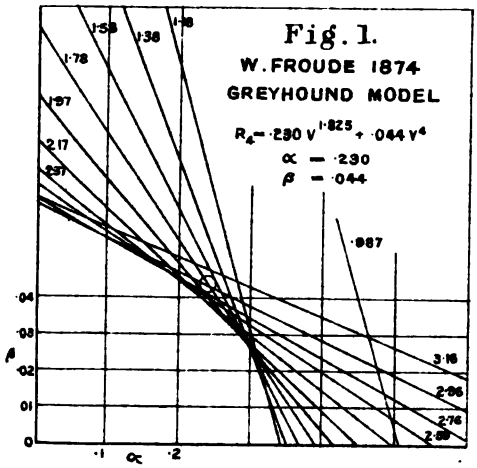


Fig. 3.

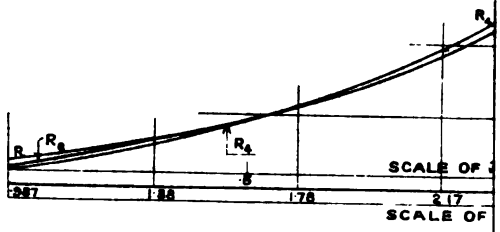
W. FROUDE 1874

GREYHOUND MODEL

CURVES OF TOTAL RESISTA

$R_4 = \cdot 00781\, SV^{1\cdot825} + 1\cdot385\, \frac{B^2D}{L^2} V^4$

$R_6 = \cdot 0116\, 8\, SV^{1\cdot825} + 1\cdot38\, \frac{B^2D}{L^3} V^6$



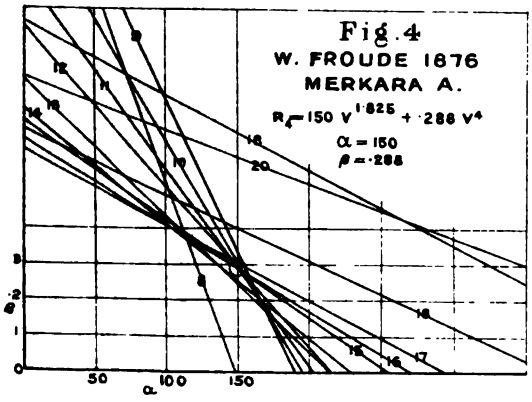


Fig. 10.

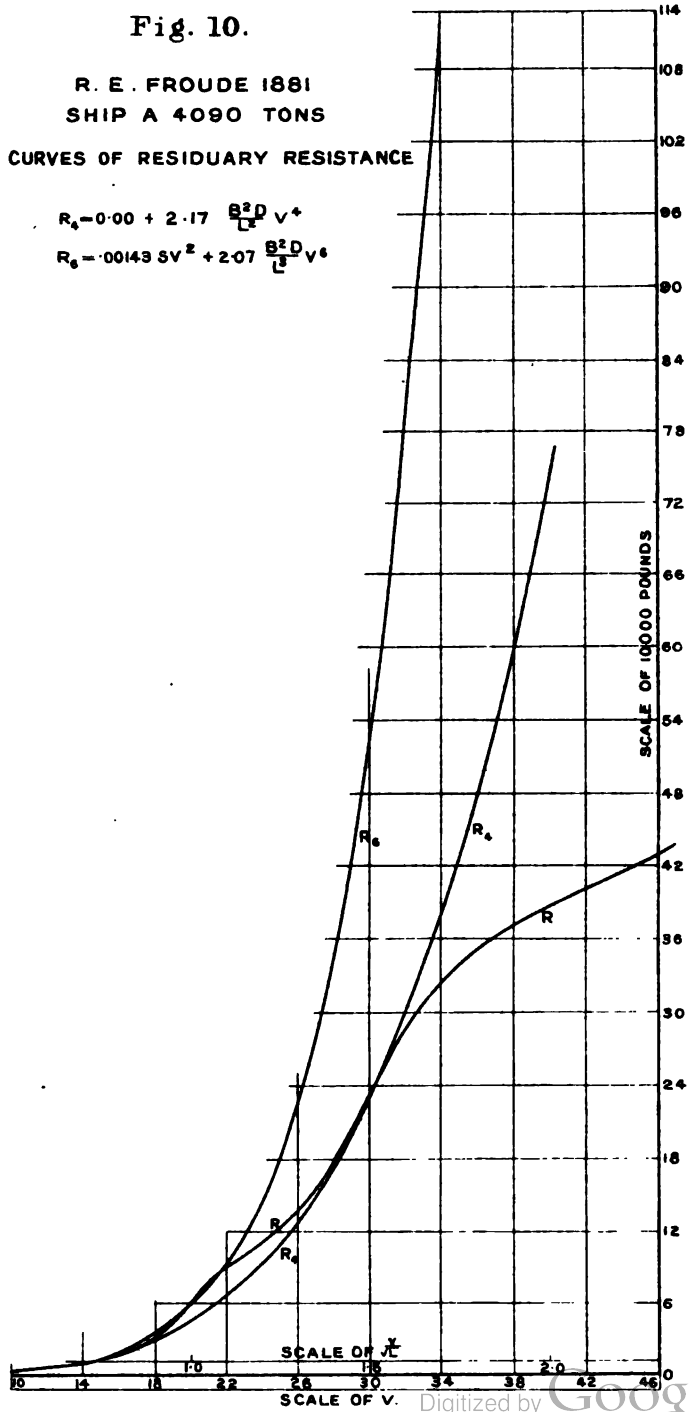
R. E. FROUDE 1881

SHIP A 4090 TONS

CURVES OF RESIDUARY RESISTANCE

$R_4 = 0\cdot00 + 2\cdot17\, \frac{B^2D}{L^2} V^4$

$R_6 = \cdot 00143\, SV^2 + 2\cdot07\, \frac{B^2D}{L^3} V^6$



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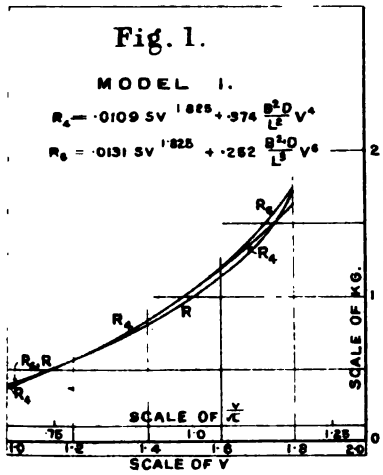

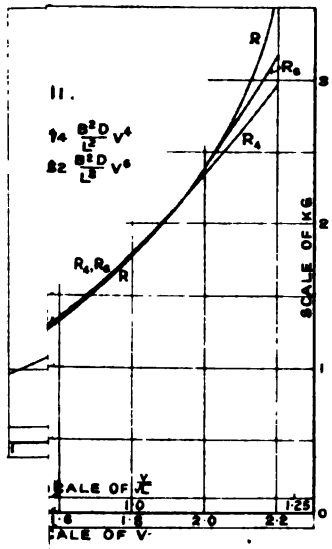

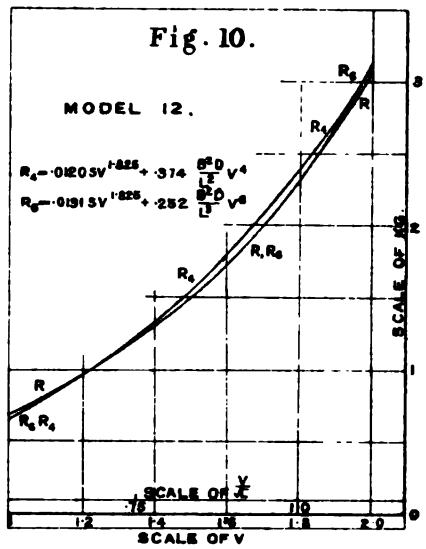

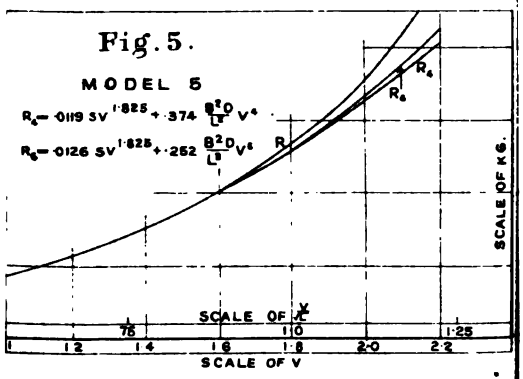

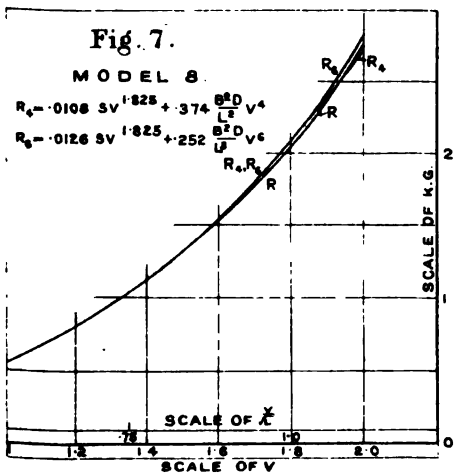

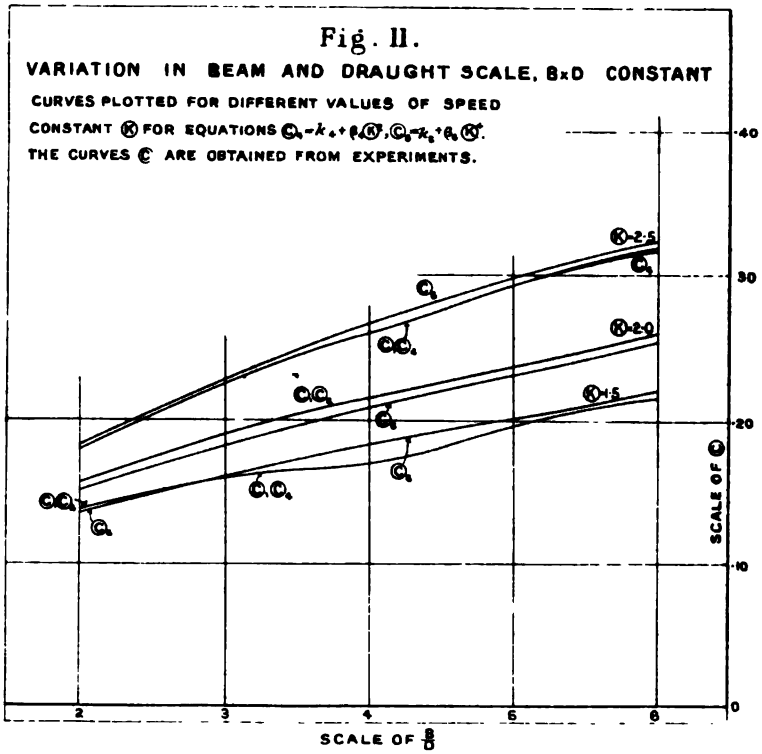
Fig. 11.
VARIATION IN BEAM AND DRAUGHT SCALE, B×D CONSTANT
CURVES PLOTTED FOR DIFFERENT VALUES OF SPEED
CONSTANT Ⓚ FOR EQUATIONS $©_4 = k_4 + \beta_4 Ⓚ^4$, $©_6 = k_6 + \beta_6 Ⓚ^6$.
THE CURVES © ARE OBTAINED FROM EXPERIMENTS.
Ⓚ=2·5
Ⓚ=2·0
Ⓚ=1·5
SCALE OF ©
SCALE OF $\frac{B}{D}$

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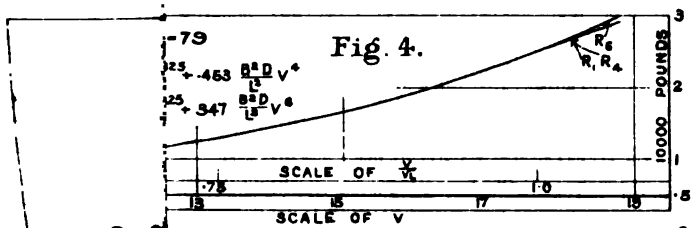

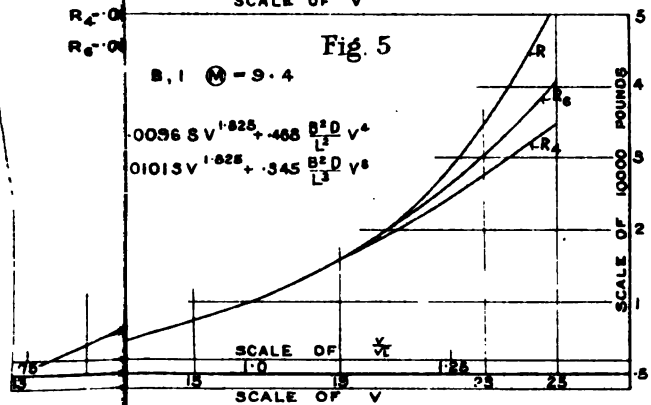

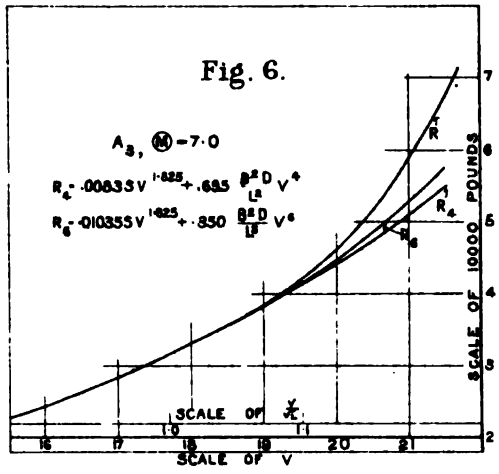

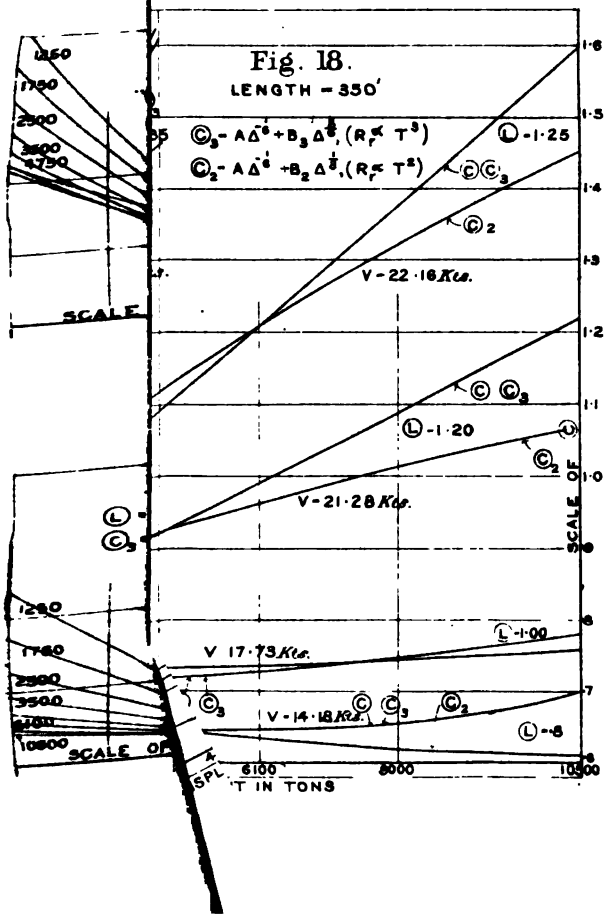

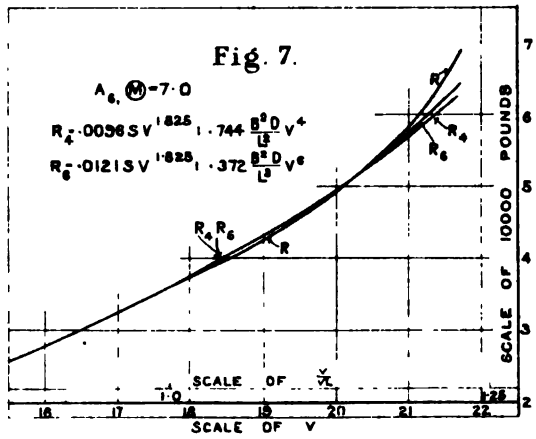

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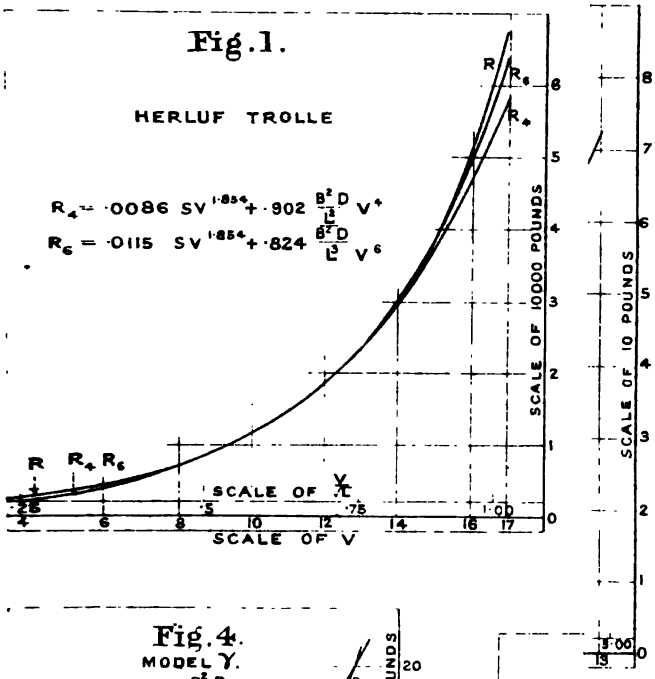


Fig. 1.
HERLUF TROLLE
$R_4 = \cdot0086\ SV^{1\cdot854} + \cdot902 \frac{B^2D}{L^3} V^4$
$R_6 = \cdot0115\ SV^{1\cdot854} + \cdot824 \frac{B^2D}{L^3} V^6$
SCALE OF 10000 POUNDS
SCALE OF 10 POUNDS
SCALE OF $\frac{V}{\sqrt{L}}$
SCALE OF V

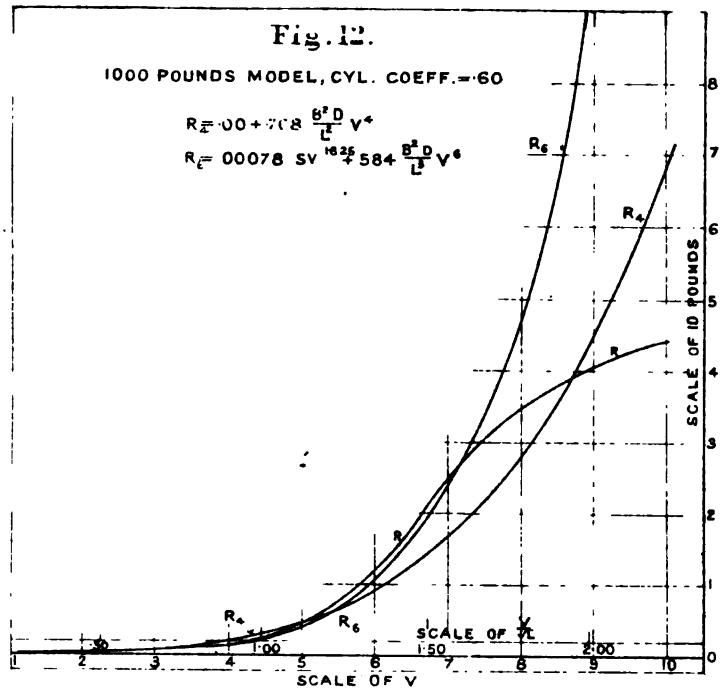


Fig. 12.
1000 POUNDS MODEL, CYL. COEFF. = ·60
$R_4 = \cdot00 + \cdot708 \frac{B^2D}{L^2} V^4$
$R_6 = \cdot00078\ SV^{1\cdot825} + \cdot584 \frac{B^2D}{L^3} V^6$
SCALE OF 10 POUNDS
SCALE OF $\frac{V}{\sqrt{L}}$
SCALE OF V

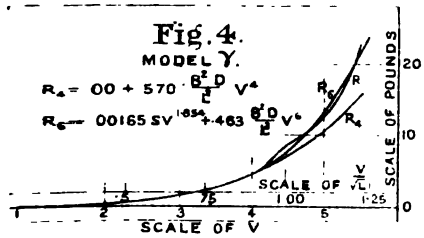


Fig. 4.
MODEL Y.
$R_4 = \cdot00 + \cdot570 \frac{B^2D}{L^2} V^4$
$R_6 = \cdot00165\ SV^{1\cdot854} + \cdot463 \frac{B^2D}{L^3} V^6$
SCALE OF POUNDS
SCALE OF $\frac{V}{\sqrt{L}}$
SCALE OF V

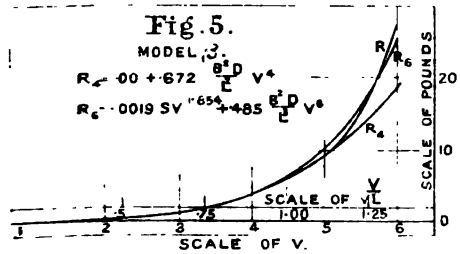
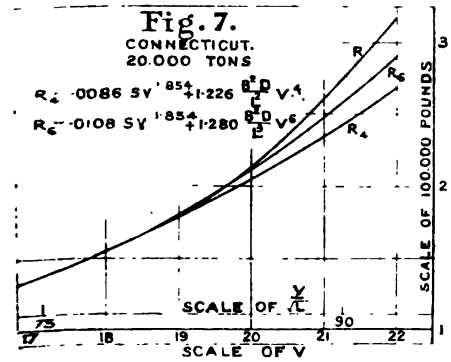
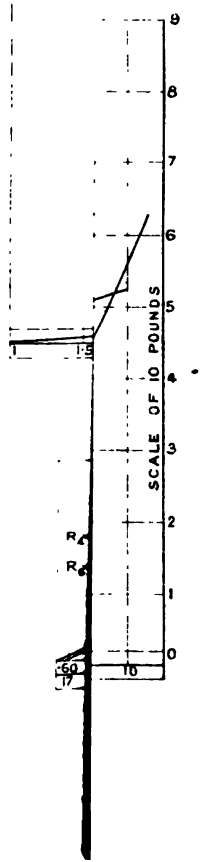


Fig. 5.
MODEL Z.
$R_4 = \cdot00 + \cdot672 \frac{B^2D}{L^2} V^4$
$R_6 = \cdot0019\ SV^{1\cdot654} + \cdot485 \frac{B^2D}{L^3} V^6$
SCALE OF POUNDS
SCALE OF $\frac{V}{\sqrt{L}}$
SCALE OF V.





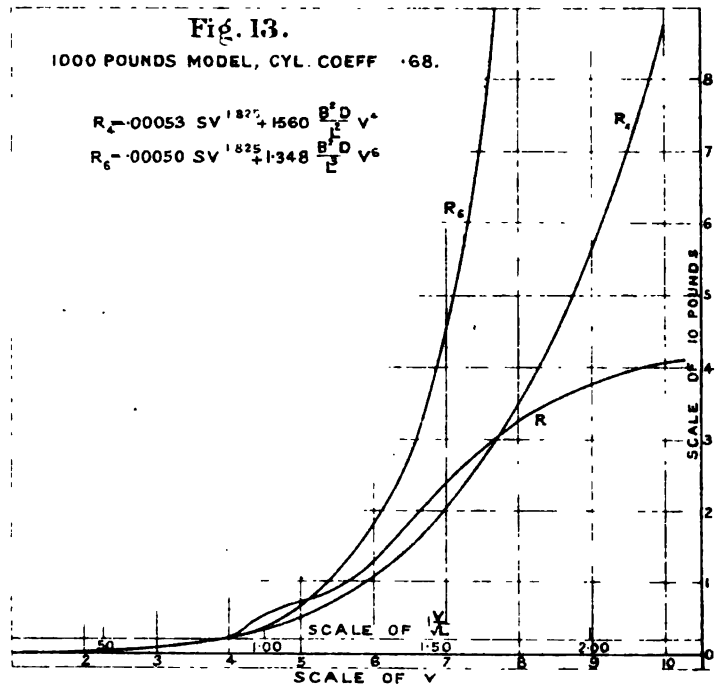


Fig. 13.
1000 POUNDS MODEL, CYL. COEFF ·68.
$R_4 = \cdot00053\ SV^{1\cdot825} + 1\cdot560 \frac{B^2D}{L^2} V^4$
$R_6 = \cdot00050\ SV^{1\cdot825} + 1\cdot348 \frac{B^2D}{L^3} V^6$
SCALE OF 10 POUNDS
SCALE OF $\frac{V}{\sqrt{L}}$
SCALE OF V

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o Illustrate Professor William Hovgaard's Paper on "An Analysis of the Resistance of Ships."

Fig. 1.

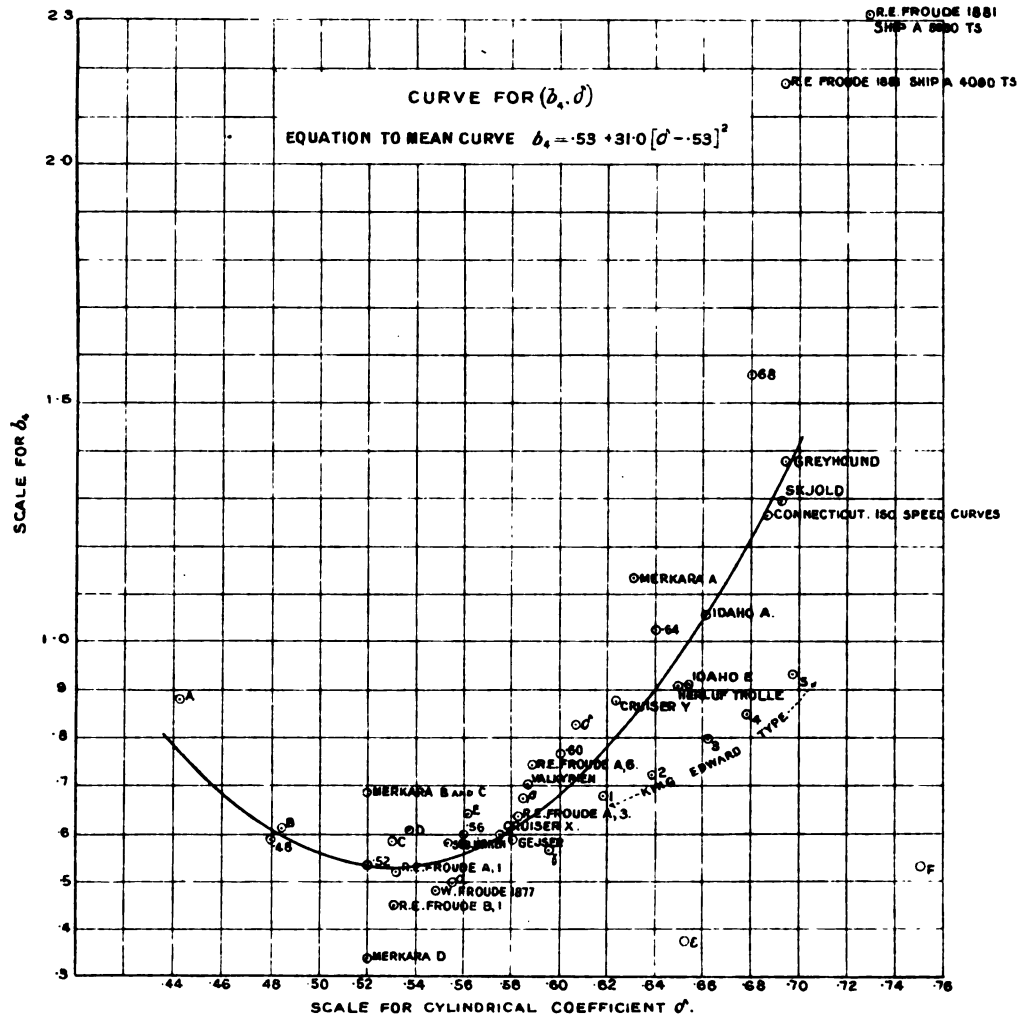


DIAGRAM SHOWING STREAM LINES AROUND SHIPS' MODELS.

(FROM NAV. CONST. D.W. TAYLOR'S PAPER, TRANS. SOC. N.A & M.E., 1907)

Fig. 2

SAN FRANCISCO

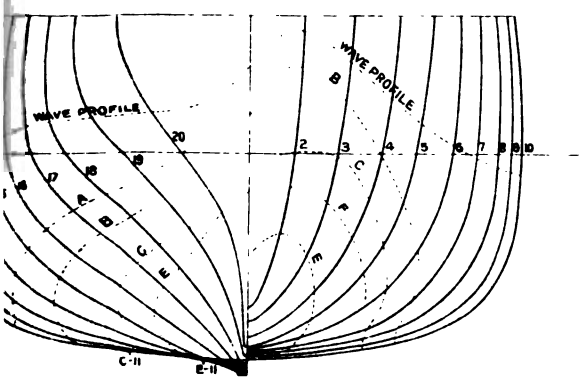
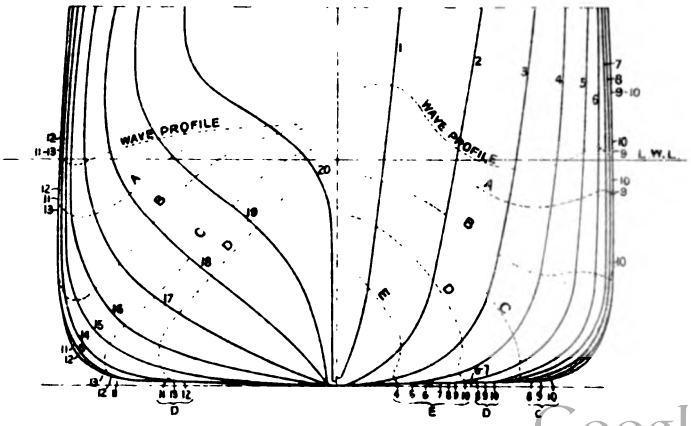


Fig. 3

STEAM COLLIER



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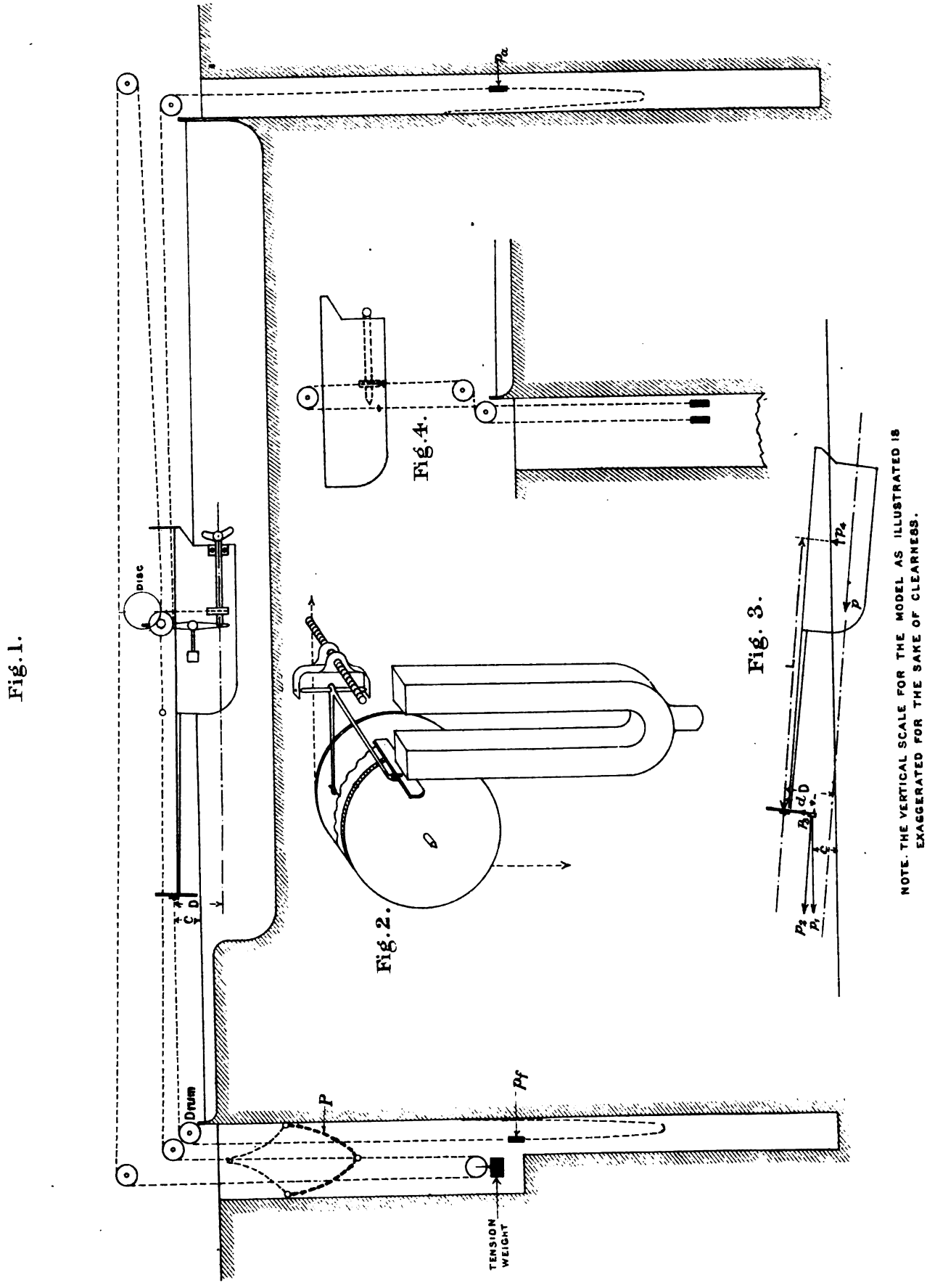
ns. Inst Naval Architects, Vol. L., 1908.

To Illustrate Herr H. Wellenkamp's Paper on "A New Method of Research Work on Fluid Resistance and Ship Propulsion"



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To Illustrate Mr. W. S. Abell's Paper on "Two Notes on Ship Calculations."

Fig. 1.

SHAPE OF FUNCTIONS FOR WATER PLANE CALCULATIONS

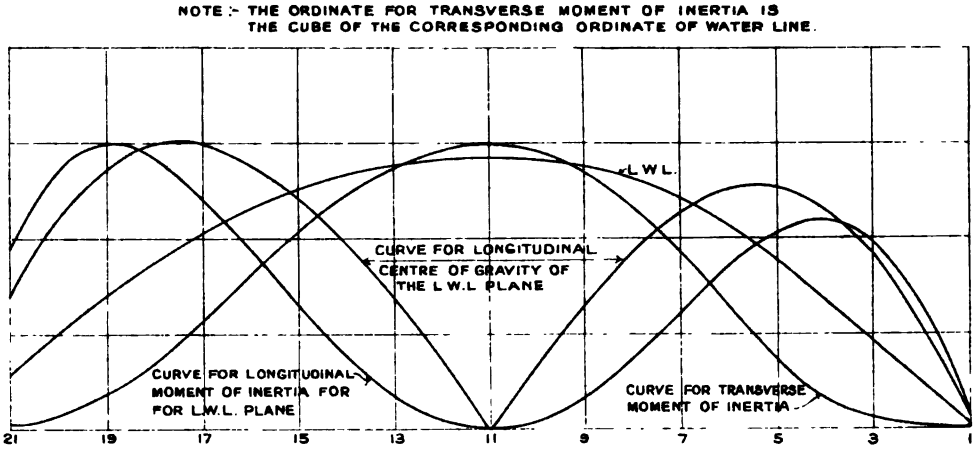

NOTE :- THE ORDINATE FOR TRANSVERSE MOMENT OF INERTIA IS THE CUBE OF THE CORRESPONDING ORDINATE OF WATER LINE.
L.W.L.
CURVE FOR LONGITUDINAL CENTRE OF GRAVITY OF THE L.W.L PLANE
CURVE FOR LONGITUDINAL MOMENT OF INERTIA FOR FOR L.W.L. PLANE
CURVE FOR TRANSVERSE MOMENT OF INERTIA
21 19 17 15 13 11 9 7 5 3 1

Fig. 2.

SHAPE OF FUNCTIONS FOR CALCULATION OF DISPLACEMENT & CENTRE OF BUOYANCY

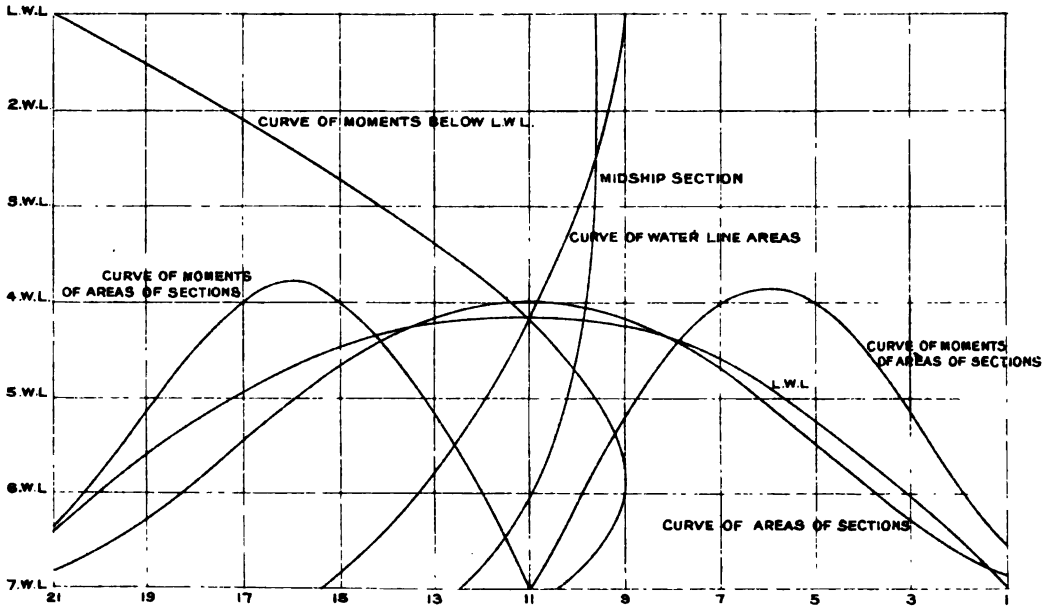
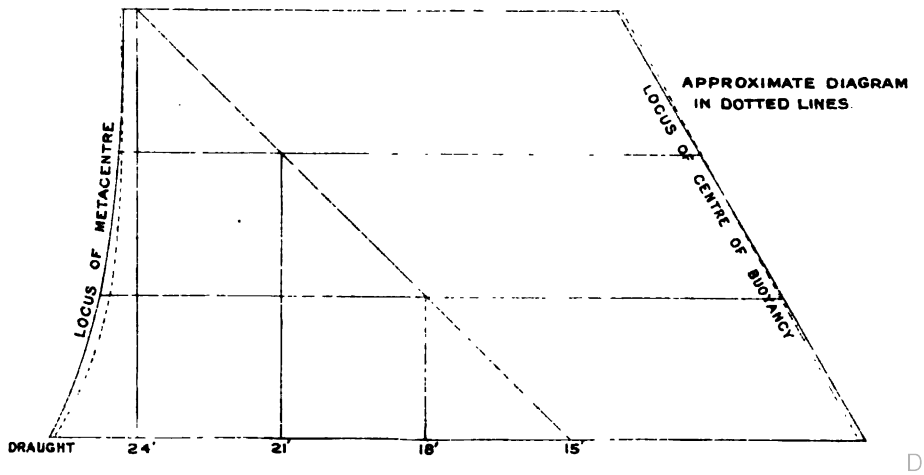


Fig. 3.

METACENTRIC DIAGRAM (IN FULL LINES)

SCALE ½" = 1 FOOT.



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To Illustrate Professor John Oliver Arnold's Paper on "Factors of Safety in Marine Engineering."

FIG. 4.

SEGREGATED AREA IN INGOT.

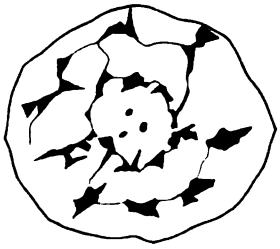

FIG. 6.
SECTIONAL PLAN AND ELEVATION OF DECARBONISED GHOST LINE IN ANNEALED FORGING.
MAGNIFIED 37½ DIAMS.

FIG. 5.
CARBONISED GHOST IN MANUFACTURED FORGING.
MAGNIFIED 125 DIAMS.

FIG. 15.
CRYSTAL OF BRITTLE WROUGHT IRON.

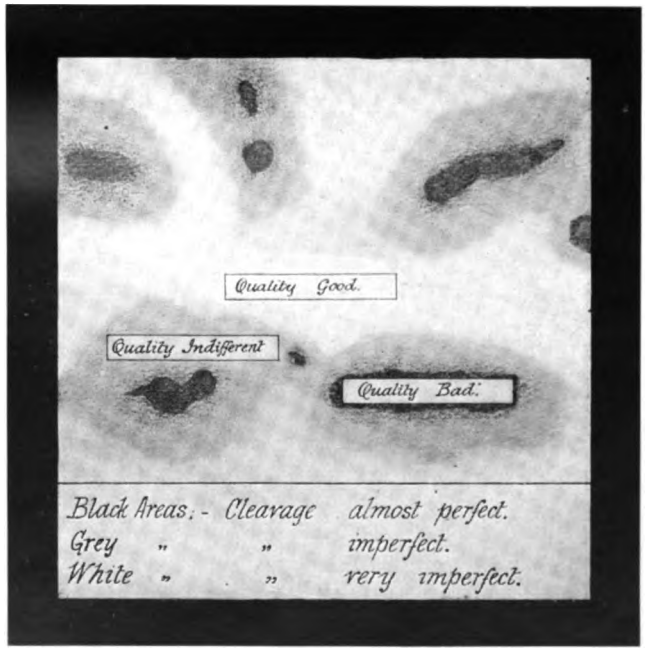

FIG. 14.

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... Engineering"

CURVES SH...
OF SA...

AND A...S.
LIMIT A...

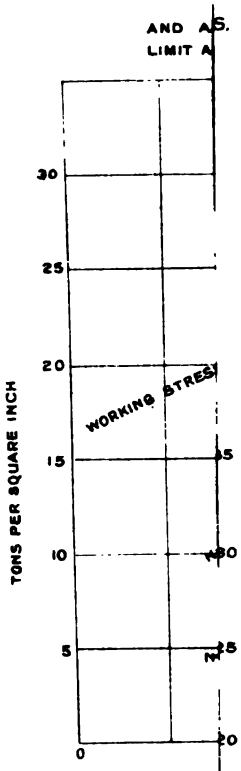
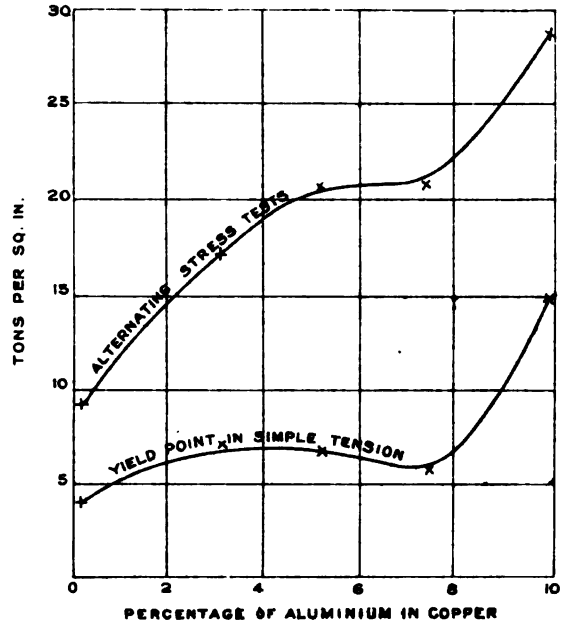


Fig. 11.

ALTERNATING STRESS TESTS. (STANTON)

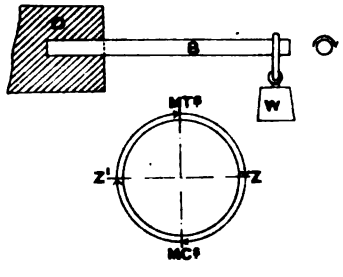
MAXIMUM LIMIT OF RESISTANCE (FOR 1 MILLION REVERSALS OF DIRECT STRESS AT A RATE OF 800 PER MINUTE) COMPARED WITH YIELD POINT IN SIMPLE TENSION



AUTHOR'S ALTERNATI...

Fig. 7.

...GRAM SHOWING NATURE AND STRESS-CYCLE OF THE WOHLER TEST.



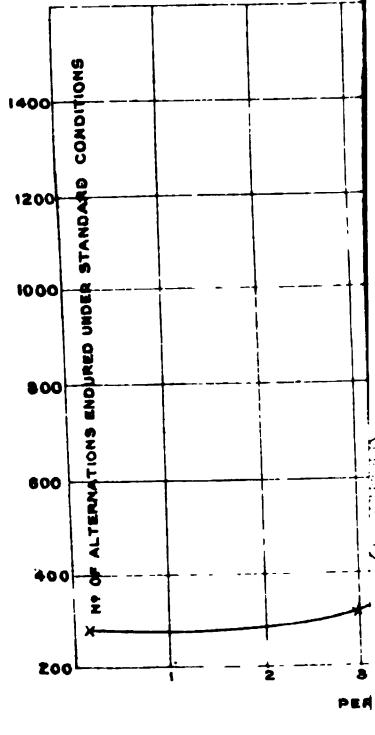


Fig. 8.

DIAGRAM SHOWING NATURE AND STRESS STRAIN LINES IN AUTHOR'S TEST

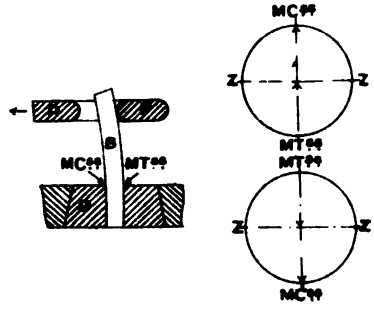


Fig. 9.

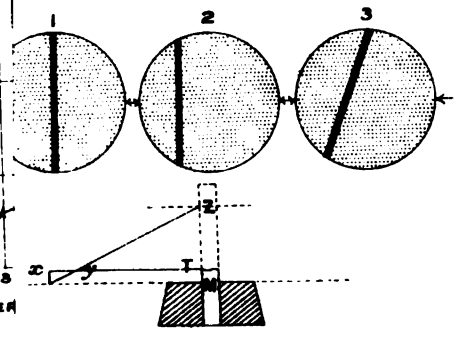
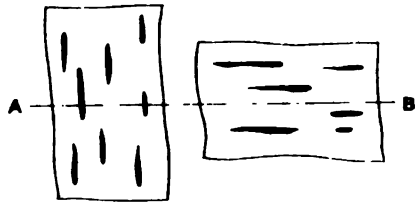


Fig. 16.

A.B. PLANE OF MAXIMUM STRESS.



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To Illustrate Mr. J. C. Dobbie's Paper on "The Modern Developments of the Mariner's Compass."

FIG. 1.
THE THOMSON COMPASS CARD.

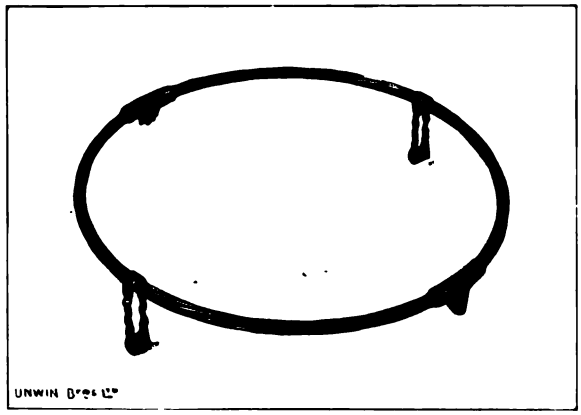

FIG. 2.
THE THOMSON GRUMMET RING.

FIG. 3.
THE KELVIN SUSPENSION.

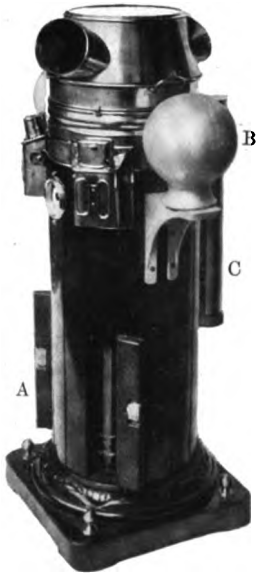

FIG. 6.

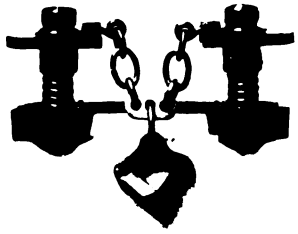

FIG. 4.
THE DOBBIE SUSPENSION.

FIG. 5.
THE DOBBIE COMPASS.

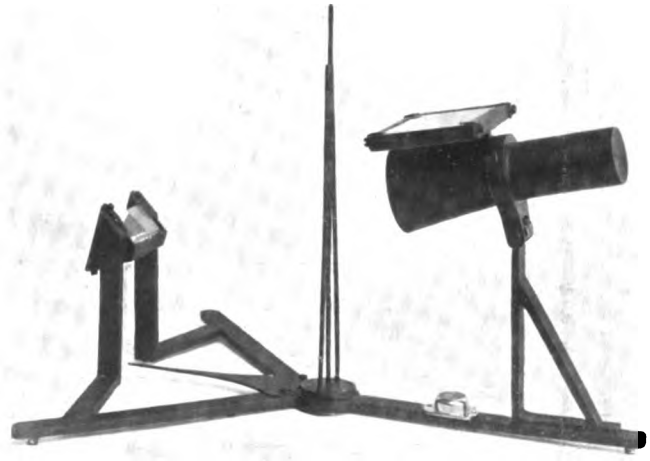

FIG. 7.
AZIMUTH INSTRUMENT.

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